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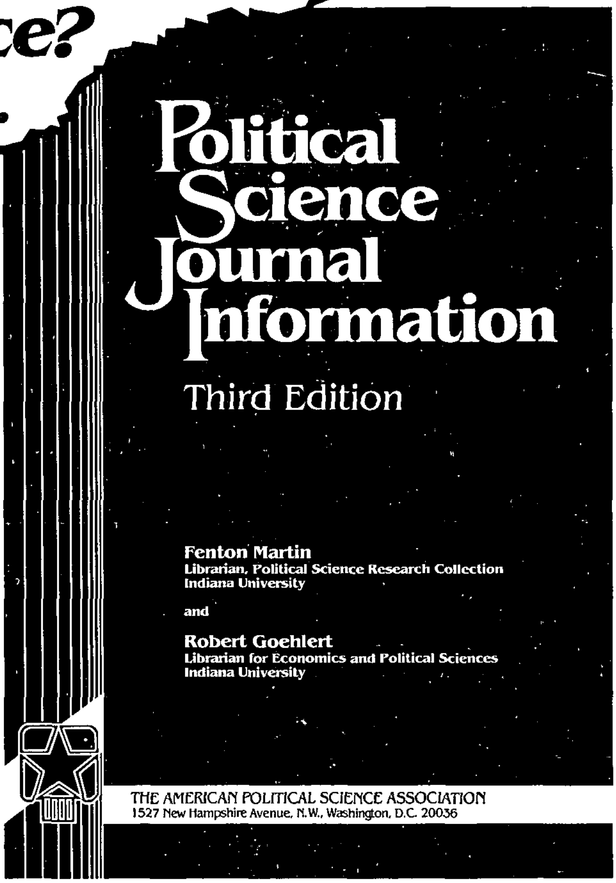

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ARTICLES

NUCLEAR MYTHS AND POLITICAL REALITIES

KENNETH N. WALTZ
*University of California
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Two pervasive beliefs have given nuclear weapons a bad name: that nuclear deterrence is highly problematic, and that a breakdown in deterrence would mean Armageddon. Both beliefs are misguided and suggest that nearly half a century after Hiroshima, scholars and policy makers have yet to grasp the full strategic implications of nuclear weaponry. I contrast the logic of conventional and nuclear weaponry to show how nuclear weapons are in fact a tremendous force for peace and afford nations that possess them the possibility of security at reasonable cost.

Nuclear weapons have been given a bad name not just by the Left, as one might have expected, but by the Center and Right as well. Throughout the long life of NATO, calls for strengthening conventional forces have been recurrently heard, reflecting and furthering debate about the wisdom of relying on nuclear deterrence. Doubts were spread more widely when McGeorge Bundy, George Kennan, Robert McNamara, and Gerald Smith published their argument for adopting a NATO policy of "no first use" (Bundy et al. 1982). From the Right came glib talk about the need to be prepared to fight a protracted nuclear war in order to "deter" the Soviet Union and proclaiming the possibility of doing so. Brigadier General Louis Guifridda, when he was director of the Federal Emergency Management Agency, well described the Reagan administration's intended nuclear stance: "The administration," he said, "categorically rejected the short war. We're trying to inject a long-war mentality" (Dowd 1984). Such statements, which scared people at home and abroad out of their wits, quickly disappeared from public discourse. Nevertheless, preparation to carry the policy through proceeded apace. In 1982 Secretary of Defense Caspar Weinberger signed the five-year Defense Guidance Plan, which was to provide the means of sustaining a nuclear war; and in March of that year an elaborate war game dubbed Ivy League "showed" that it could be done (Pringle and Arkin 1983, 22–40). Finally, in March of 1983 President Reagan offered his vision of a world in which defensive systems would render nuclear weapons obsolete.

With their immense destructive power, nuclear weapons are bound to make people uneasy. Decades of fuzzy thinking in high places about what deterrence is, how it works, and what it can and cannot do have deepened the nuclear malaise. Forty-some years after the first atomic bombs fell on Japan, we have yet to come to grips with the strategic implications of nuclear weapons. I apply nuclear reasoning to military policy and in doing so contrast the logic of conventional and nuclear weapons.

Uneasiness over nuclear weapons and the search for alternative means of security stem in large measure from widespread

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failure to understand the nature and requirements of deterrence. Not unexpectedly, the language of strategic discourse has deteriorated over the decades. This happens whenever discussion enters the political arena, where words take on meanings and colorations reflecting the preferences of their users. Early in the nuclear era *deterrence* carried its dictionary definition, dissuading someone from an action by frightening that person with the consequences of the action. To deter an adversary from attacking one need have only a force that can survive a first strike and strike back hard enough to outweigh any gain the aggressor had hoped to realize. Deterrence in its pure form entails no ability to defend; a deterrent strategy promises not to fend off an aggressor but to damage or destroy things the aggressor holds dear. Both defense and deterrence are strategies that a status quo country may follow, hoping to dissuade a state from attacking. They are different strategies designed to accomplish a common end in different ways, using different weapons differently deployed. Wars can be prevented, as they can be caused, in various ways.

Deterrence antedates nuclear weapons, but in a conventional world deterrent threats are problematic. Stanley Baldwin warned in the middle 1930s when he was prime minister of England that the bomber would always get through, a thought that helped to demoralize England. It proved seriously misleading in the war that soon followed. Bombers have to make their way past fighter planes and through ground fire before finding their targets and hitting them quite squarely. Nuclear weapons purify deterrent strategies by removing elements of defense and war-fighting. Nuclear warheads eliminate the necessity of fighting and remove the possibility of defending, because only a small number of warheads need to reach their targets.

Ironically, as multiplication of missiles increased the ease with which destructive blows can be delivered, the distinction between deterrence and defense began to blur. Early in President Kennedy's administration, Secretary McNamara began to promote a strategy of Flexible Response, which was halfheartedly adopted by NATO in 1967. Flexible Response calls for the ability to meet threats at all levels from irregular warfare to conventional warfare to nuclear warfare. In the 1970s and 1980s more and more emphasis was placed on the need to fight and defend at all levels in order to "deter." The melding of defense, war-fighting, and deterrence overlooks a simple truth about nuclear weapons proclaimed in the book title *The Absolute Weapon* (Brodie 1946). Nuclear weapons can carry out their deterrent task no matter what other countries do. If one nuclear power were able to destroy almost all of another's strategic warheads with practical certainty or defend against all but a few strategic warheads coming in, nuclear weapons would not be absolute. But because so much explosive power comes in such small packages, the invulnerability of a sufficient number of warheads is easy to achieve and the delivery of fairly large numbers of warheads impossible to thwart, both now and as far into the future as anyone can see. The absolute quality of nuclear weapons sharply sets a nuclear world off from a conventional one.

What Deters?

Most discussions of deterrence are based on the belief that deterrence is difficult to achieve. In the Eisenhower years "massive retaliation" was the phrase popularly used to describe the response we would supposedly make to a Soviet Union attack. Deterrence must be difficult if the threat of massive retaliation is required to achieve it. As the Soviet Union's arsenal grew, MAD (mutual assured destruction) became the acronym of choice, thus preserving the notion that deterrence depends on being willing and able to destroy much, if not most, of a country.

That one must be able to destroy a country in order to deter it is an odd notion, though of distinguished lineage. During the 1950s emphasis was put on the *massive* in *massive retaliation.* Beginning in the 1960s the emphasis was put on the *assured destruction* in the doctrine of MAD. Thus viewed, deterrence becomes a monstrous policy, as innumerable critics have charged. One quotation can stand for many others. In a warning to NATO defense ministers that became famous, Henry Kissinger counseled the European allies not to keep "asking us to multiply strategic assurances that we cannot possibly mean or if we do mean, we should not want to execute because if we execute, we risk the destruction of civilization" (1981, 240). The notion that the failure of deterrence would lead to national suicide or to mutual annihilation betrays a misunderstanding of both political behavior and nuclear realities.

Introducing the Eisenhower administration's New Look policy in January of 1954, John Foster Dulles gave the impression that aggression anywhere would elicit heavy nuclear retaliation. Just three months later, he sensibly amended the policy. Nuclear deterrence, Dulles and many others quickly came to realize, works not against minor aggression at the periphery, but only against major aggression at the center, of international politics. Moreover, to deter major aggression, Dulles now said, "the probable hurt" need only "outbalance the probable gain" (1954, 359). Like Brodie before him, Dulles based deterrence on the principle of proportionality: "Let the punishment fit the crime."

What would we expect the United States to do if the Soviet Union launched a major conventional attack against vital U.S. interests—say, in Western Europe? Military actions have to be related to an objective. Because of the awesome power of nuclear weapons, the pressure to use them in ways that achieve the objective at hand while doing and suffering a minimum of destruction would be immense. It is preposterous to think that if a Soviet attack broke through NATO's defenses, the United States would strike thousands of Soviet military targets or hundreds of Soviet cities. Doing so would serve no purpose. Who would want to make a bad situation worse by launching wantonly destructive attacks on a country that can strike back with comparable force, or, for that matter, on a country that could not do so? In the event, we might strike a target or two—military or industrial—chosen to keep casualties low. If the Soviet Union had run the preposterous risk of attacking the center of Europe believing it could escape retaliation, we would thus show them that they were wrong while conveying the idea that more would follow if they persisted. Among countries with abundant nuclear weapons, none can gain an advantage by striking first. The purpose of demonstration shots is simply to remind everyone—should anyone forget—that catastrophe threatens. Some people purport to believe that if a few warheads go off, many will follow. This would seem to be the least likely of all the unlikely possibilities. That no country gains by destroying another's cities and then seeing a comparable number of its own destroyed in return is obvious to everyone.

Despite widespread beliefs to the contrary, deterrence does not depend on destroying cities. Deterrence depends on what one *can* do, not on what one *will* do. What deters is the fact that we can do as much damage to them as we choose, and they to us. The country suffering the retaliatory attack cannot limit the damage done to it; only the retaliator can do that.

With nuclear weapons, countries need threaten to use only a small amount of force. This is so because once the willingness to use a little force is shown, the adversary knows how easily more can be added. This is not true with conventional weapons. Therefore, it is often useful for a country to threaten to use great force if conflict should lead to war. The stance may be intended as a deterrent one, but the ability to carry the threat through is problematic. With conventional weapons, countries tend to emphasize the first phase of war. Striking hard to achieve a quick victory may decrease the cost of war. With nuclear weapons, political leaders worry not about what may happen in the first phase of fighting but about what may happen in the end. As Clausewitz wrote, if war should ever approach the absolute, it would become "imperative . . . not to take the first step without considering what may be the last" (1976, 584).

Since war now approaches the absolute, it is hardly surprising that President Kennedy echoed Clausewitz' words during the Cuban Missile Crisis of 1962. "It isn't the first step that concerns me," he said, "but both sides escalating to the fourth and fifth step—and we don't go to the sixth because there is no one around to do so" (R. Kennedy 1969, 98). In conventional crises, leaders may sensibly seek one advantage or another. They may bluff by threatening escalatory steps they are in fact unwilling to take. They may try one stratagem or another and run considerable risks. Since none of the parties to the struggle can predict what the outcome will be, they may have good reason to prolong crises, even crises entailing the risk of war. A conventional country enjoying military superiority is tempted to use it before other countries right the military balance. A nuclear country enjoying superiority is reluctant to use it because no one can promise the full success of a disarming first strike. As Henry Kissinger retrospectively said of the Cuban Missile Crisis, the Soviet Union had only "60–70 truly strategic weapons while we had something like 2,000 in missiles and bombs." But, he added, "with some proportion of Soviet delivery vehicles surviving, the Soviet Union could do horrendous damage to the United States" (Kissinger 1979, 18). In other words, we could not be sure that our two thousand weapons would destroy almost all of their sixty or seventy. Even with numbers immensely disproportionate, a small force strongly inhibits the use of a large one.

The catastrophe promised by nuclear war contrasts sharply with the extreme difficulty of predicting outcomes among conventional competitors. This makes one wonder about the claimed dependence of deterrence on perceptions and the alleged problem of credibility. In conventional competitions, the comparative qualities of troops, weaponry, strategies, and leaders are difficult to gauge. So complex is the fighting of wars with conventional weapons that their outcomes have been extremely difficult to predict. Wars start more easily because the uncertainties of their outcomes make it easier for the leaders of states to entertain illusions of victory at supportable cost. In contrast, contemplating war when the use of nuclear weapons is possible focuses one's attention not on the probability of victory but on the possibility of annihilation. Because catastrophic outcomes of nuclear exchanges are easy to imagine, leaders of states will shrink in horror from initiating them. With nuclear weapons, stability and peace rest on easy calculations of what one country can do to another. Anyone—political leader or man in the street—can see that catastrophe lurks if events spiral out of control and nuclear warheads begin to fly. The problem of the credibility of deterrence, a big worry in a conventional world, disappears in a nuclear one.

Yet the credibility of deterrence has been a constant U.S. worry. The worry is a hangover from the 1930s. Concern over credibility, and the related efforts to show resolve in crises or wars where only peripheral interests are at stake were reinforced because the formative experiences of most of the policy makers of the 1950s and 1960s took place in the 1930s. In rearming Germany, in reoccupying the Rhineland, in annexing Austria, and in dismantling Czechoslovakia, Hitler went to the brink and won. "We must not let that happen again" was the lesson learned, but in a nuclear world the lesson no longer applies. Despite rhetoric to the contrary, practice accords with nuclear logic because its persuasive force is so strong, and the possible consequences of ignoring it so grave. Thus, John Foster Dulles, who proclaimed that maintaining peace requires the courage to go the brink of war, shrank from the precipice during the Hungarian uprising of 1956. And so it has been every time that events even remotely threatened to get out of hand at the center of international politics.

Still, strategists' and commentators' minds prove to be impressively fertile. The imagined difficulties of deterrence multiply apace. One example will do: Paul Nitze argued in the late 1970s that, given a certain balance of strategic forces, the Soviet Union's supposed goal of world domination, and its presumed willingness to run great risks, the Soviet Union might launch a first strike against our land-based missiles, our bombers on the ground, and our strategic submarines in port. The Soviet Union's strike would tilt the balance of strategic forces sharply against us. Rather than retaliate, our president might decide to acquiesce; that is, we might be self-deterred (1988, 357–60). Nitze's scenario is based on faulty assumptions, unfounded distinctions, and preposterous notions about how governments behave. Soviet leaders, according to him, may have concluded from the trend in the balance of nuclear forces in the middle 1970s that our relatively small warheads and their civil defense would enable the Soviet Union to limit the casualties resulting from our retaliation to 3% or 4% of their population. Their hope for such a "happy" outcome would presumably rest on the confidence that their first strike would be well timed and accurate and that their intelligence agencies would have revealed the exact location of almost all of their intended targets. In short, their leaders would have to believe that all would go well in a huge, unrehearsed missile barrage, that the United States would fail to launch on warning, and that if by chance they had failed to "deter our deterrent," they would still be able to limit casualties to only ten million people or so.[1] But how could they entertain such a hope when by Nitze's own estimate their first strike would have left us with two thousand warheads in our submarine force in addition to warheads carried by surviving bombers?

Nitze's fear rested on the distinction between counterforce strikes and countervalue strikes—strikes aimed at weapons and strikes aimed at cities. Because the Soviet Union's first strike would be counterforce, any U.S. president would seemingly have good reason to refrain from retaliation, thus avoiding the loss of cities still held hostage by the Soviet Union's remaining strategic forces. But this thought overlooks the fact that once strategic missiles numbered in the low hundreds are fired, the counterforce-countervalue distinction blurs. One would no longer know what the attacker's intended targets might be. The Soviet Union's counterforce strike would require that thousands, not hundreds, of warheads be fired. Moreover, the extent of their casualties, should we decide to retaliate, would depend on how many of our warheads we chose to fire, what targets we aimed at, and whether we used ground bursts to increase fallout. Several hundred warheads could destroy either the United States or the Soviet Union as ongoing societies. The assumptions made in the effort to make a Soviet first strike appear possible are ridiculous. How could the Soviet Union—or any country, for that matter—somehow bring itself to run stupendous risks in the presence of nuclear weapons? What objectives might its leaders seek that could justify the risks entailed? Answering these questions sensibly leads one to conclude that deterrence is deeply stable. Those who favor increasing the strength of our strategic forces, however, shift to a different question. "The crucial question," according to Nitze, "is whether a future U.S. president should be left with only the option of deciding within minutes, or at most within two or three hours, to retaliate after a counterforce attack in a manner certain to result not only in military defeat for the United States but in wholly disproportionate and truly irremediable destruction to the American people" (1988, 357). One of the marvels of the nuclear age is how easily those who write about the unreliability of deterrence focus on the retaliator's possible inhibitions and play down the attacker's obvious risks. Doing so makes deterrence seem hard and leads to arguments for increasing our military spending in order "to deny the Soviet Union the possibility of a successful war-fighting capability" (1988, 360), a strategic capability that the Soviet Union has never remotely approached.

We do not need ever-larger forces to deter. Smaller forces, so long as they are invulnerable, would be quite sufficient. Yet the vulnerability of fixed, land-based missiles has proved worrisome. Those who do the worrying dwell on the vulnerability of one class of weapon. The militarily important question, however, is not about the vulnerability of one class of weapon but about the vulnerability of a whole strategic-weapons system. Submarine-launched missiles make land-based missiles invulnerable since destroying only the latter would leave thousands of strategic warheads intact. To overlook this again reflects conventional thinking. In the absence of a dominant weapon, the vulnerability of one weapon or another may be a big problem. If the means of protecting sea-lanes of communications were destroyed, for example, we would be unable to deploy and support troops abroad. The problem disappears in a nuclear world. Destroying a portion of one's strategic force means little if sufficient weapons for deterrence survive.

Thinking about deterrence is often faulted for being abstract and deductive, for not being grounded in experience. The criticism is an odd one, since all statements about the military implications of nuclear weapons are inferred from their characteristics. Deterrers from Brodie onward have drawn conclusions from the all-but-unimaginable increase in easily delivered firepower that nuclear warheads embody. Those who in the nuclear era apply lessons learned in conventional warfare make the more problematic claim that despite profound changes in military technology the classic principles of warfare endure (Rose 1980, 102-106). We all, happily, lack the benefit of experience. Moreover, just as deterrent logic is abstract and deductive, so too are the weaknesses attributed to it. Scenarios showing how deterrence might fail are not only abstract but also far-fetched. Deterrence rests on simple propositions and relies on forces obviously sufficient for their purpose.

Deterring the Soviet Union

Underlying much of the concern about the reliability of nuclear deterrence is the conviction that the Soviet Union is especially hard to deter. Three main reasons are given for believing this. First, the Soviet Union's ambitions are said to be unlimited. In 1984 Secretary of Defense Caspar Weinberger, when asked why the Soviet Union armed itself so heavily, answered the question bluntly, "World domination, it's that simple" (Rosenthal 1989). Second, her military doctrine seemed to contemplate the possibility of fighting and winning combined conventional and nuclear war, while rejecting the doctrine of deterrence. Third, the Soviet Union has appeared to many people in the West to be striving for military superiority.

These three points make a surprisingly weak case, even though it has been widely accepted. Ambitions aside, looking at the Soviet Union's behavior one is impressed with its caution when acting where conflict might lead to the use of major force. Leaders of the Soviet Union may hope that they can one day turn the world to communism. Although the Soviet Union's intentions may be extraordinary, her behavior has not been. Everyone agrees that except in the military sector, the Soviet Union is the lagging competitor in a two-party race. The Soviet Union has been opportunistic and disruptive, but one expects the lagging party to score a point or two whenever it can. The Soviet Union has not scored many. Her limited international successes should not obscure the fact that what the Soviet Union has done mostly since 1948 is lose.

The second point rests on basic misunderstandings about deterrence. It has often been argued that we could not rely on deterrence when the Soviet Union was rejecting the doctrine. One of the drawbacks of the "theory" of assured destruction, according to Henry Kissinger, was that "the Soviets did not believe it" (1981, 238). The efficacy of nuclear deterrence, however, does not depend on anyone's accepting it. Secretaries of defense nevertheless continue to worry that Soviet values, perceptions, and calculations may be different from ours. Thus, Secretary of Defense Harold Brown, worried by the Soviets' emphasis "on the acquisition of war-winning capabilities," concluded that we must "continue to adapt and update our countervailing capabilities so that the Soviets will clearly understand that we will never allow them to use their nuclear forces to achieve any aggressive aim at an acceptable cost" (1980, 83).

The belief that the Soviet Union's having an aggressive military doctrine makes her especially hard to deter is another hangover from conventional days. Germany and Japan in the 1930s were hard to deter, but then the instruments for deterrence were not available. We can fairly say that their leaders were less averse to running risks than most political leaders are. But that is no warrant for believing that had they been confronted with second-strike nuclear forces, they would have been so foolhardy as to risk the sudden destruction of their countries. The decision to challenge the vital interests of a nuclear state, whether by major conventional assault or by nuclear first strike, would be a collective decision involving a number of political and military leaders. One would have to believe that a whole set of leaders might suddenly go mad. Rulers like to continue to rule. Except in the relatively few countries of settled democratic institutions, one is struck by how tenaciously rulers cling to power. We have no reason to expect Russian leaders to be any different. The notion that Russian leaders might risk losing even a small number of cities by questing militarily for uncertain gains is fanciful. Malenkov and Khrushchev lost their positions for lesser failures.

With conventional weapons a status quo country must ask itself how much power it must harness to its policy in order to dissuade an especially aggressive state from striking. Countries willing to run high risks are hard to dissuade. The varied qualities of governments and the temperaments of leaders have to be carefully weighed. In a nuclear world any state will be deterred by another state's second-strike forces. One need not become preoccupied with the characteristics of the state that is to be deterred or scrutinize its leaders.

The third worry remains: the Soviet Union's seeming aspiration for military superiority. One might think that the worry should have run the other way through most of the years of the Cold War. In the nuclear business the United States moved from monopoly to superiority. In the late fifties Khrushchev deeply cut conventional arms, and the Soviet Union failed to produce strategic warheads and missiles as rapidly as we had expected. Nevertheless, the Kennedy administration undertook the largest peacetime military buildup the world had yet seen in both nuclear and conventional weaponry. We forged ahead so far strategically that Robert McNamara thought the Soviet Union would not even try to catch up. "There is," he said, "no indication that the Soviets are seeking to develop a strategic nuclear force as large as ours" (Interview, *U.S. News and World Report*, 12 April 1976, 52). The Soviet Union's giving up would have been historically unprecedented. Instead, the Soviet Union did try to compete, but to catch up with the United States was difficult. In the 1970s, the decade in which we are told the Soviet Union moved toward superiority (or, according to President Reagan, achieved it), the United States in fact added more nuclear warheads to its arsenal than the Soviet Union did.

We have exaggerated the strength of the Soviet Union; and they, no doubt, have exaggerated ours. One may wonder whether the Soviet Union ever thought itself superior or believed it could become so. Americans easily forget that the Soviet Union has the strategic weapons of the United States, Britain, France, and China pointed at it and sees itself threatened from the East as well as the West. More fundamentally, continued preoccupation with denying "superiority" to the Soviet Union, if not seeking it ourselves, suggests that a basic strategic implication of nuclear weapons is yet to be appreciated. So long as two or more countries have second-strike forces, to compare them is pointless. If no state can launch a disarming attack with high confidence, force comparisons become irrelevant. For deterrence one asks how much is enough, and enough is defined as "having a second-strike capability." This does not imply that a deterrent force deters everything, but rather that beyond a certain level of capability, additional forces provide no additional coverage for one party and pose no additional threat to others. The United States and the Soviet Union have long had second-strike forces, with neither able to launch a disarming first strike against the other. Two countries with second-strike forces have the same amount of strategic power—since, short of attaining a first-strike capability, adding more weapons does not change the effective military balance.

Why Nuclear Weapons Dominate Strategy

Deterrence is easier to contrive than most strategists have believed. With conventional weapons, a number of strategies are available, strategies combining and deploying forces in different ways. Strategies may do more than weapons to determine the outcomes of wars. Nuclear weapons are different; they dominate strategies. As Brodie clearly saw, the effects of nuclear weapons derive not from any particular design for their employment in war but simply from their presence (1973, 412). Indeed, in an important sense, nuclear weapons eliminate strategy. If one thinks of strategies as being designed for defending national objectives or for gaining them by military force and as implying a choice about how major wars will be fought, nuclear weapons make strategy obsolete. Nevertheless, the conviction that the only reliable deterrent force is one able to win a war or at least end up in a better position than the Soviet Union is widespread. Linton F. Brooks, while a captain in the U.S. Navy, wrote that "war is the ultimate test of any strategy; a strategy useless in war cannot deter" (1988, 580; see also Howard 1981, 15).

NATO policy well illustrates the futility of trying to transcend deterrence by fashioning war-fighting strategies. The supposed difficulties of extending deterrence to cover major allies has led some to argue that we require nuclear superiority, that we need nuclear war-fighting capabilities, and that we must build up our conventional forces. Once the Soviet Union achieved nuclear parity, confidence in our extended deterrent declined in the West. One wonders whether it did in the East. Denis Healey once said that one chance in a hundred that a country will retaliate is enough to deter an adversary, although not enough to reassure an ally. Many have repeated his statement; but none, I believe, has added that reassuring allies is unnecessary militarily and unwise politically. Politically, allies who are unsure of one another's support have reason to work harder for the sake of their own security. Militarily, deterrence requires only that conventional forces be able to defend long enough to determine that an attack is a major one and not merely a foray. For this, a trip wire force as envisioned in the 1950s, with perhaps fifty thousand U.S. troops in Europe, would be sufficient. Beyond that, deterrence requires only that forces be invulnerable and that the area protected be of manifestly vital interest. West European countries can be counted on to maintain forces of trip wire capability.

Nuclear weapons strip conventional forces of most of their functions. Bernard Brodie pointed out that in "a total war" the army "might have no function at all" (1957, 115). Herman Kahn cited "the claim that in a thermonuclear war it is important to keep the sea lanes open" as an example of the "quaint ideas" still held by the military (1960, 38). Conventional forces have only a narrow role in any confrontation between nuclear states over vital interests, since fighting beyond the trip wire level serves no useful purpose. Enlarging conventional capabilities does nothing to strengthen deterrence. Strategic stalemate does shift military competition to the tactical level. But one must add what is usually omitted: nuclear stalemate limits the use of conventional forces and reduces the extent of the gains one can seek without risking devastation. For decades U.S. policy has nevertheless aimed at raising the nuclear threshold in Europe. Stronger conventional forces would presumably enable NATO to sustain a longer war in Europe at higher levels of violence. At some moment in a major war, however, one side or the other—or perhaps both—would believe itself to be losing. The temptation to introduce nuclear weapons might then prove irresistible, and they would be fired in the chaos of defeat with little chance of limited and discriminant use. Early use would promise surer control and closer limitation of damage. In a nuclear world a conventional war-fighting strategy would appear to be the worst possible one, more dangerous than a strategy of relying on deterrence.

Attempts to gain escalation dominance, like efforts to raise the nuclear threshold, betray a failure to appreciate the strategic implications of nuclear weapons. Escalation dominance, so it is said, requires a "seamless web of capabilities" up and down "the escalation ladder." Earlier, it had been thought that the credibility of deterrence would be greater if some rungs of the escalation ladder were missing. The inability to fight at some levels would make the threat to use higher levels of force easy to credit. But again, since credibility is not a problem, this scarcely matters militarily. Filling in the missing rungs neither helps nor hurts. Escalation dominance is useful for countries contending with conventional weapons only. Dominance, however, is difficult to achieve in the absence of a decisive weapon. Among nuclear adversaries the question of dominance is pointless because one second-strike force cannot dominate another. Since strategic nuclear weapons will always prevail, the game of escalation dominance cannot be played. Everyone knows that anyone can quickly move to the top rung of the ladder. Because anyone can do so, all of the parties in a serious crisis have an overriding incentive to ask themselves one question: How can we get out of this mess without nuclear warheads' exploding? The presence of nuclear weapons forces them to figure out how to deescalate, not how to escalate.

To gain escalation dominance, if that were imaginable, would require the ability to fight nuclear wars. War-fighting strategies imply that nuclear weapons are not absolute but relative, so that the country with more and better nuclear weapons could in some unspecified way prevail. No one, however, has shown how such a war could be fought. Indeed, Desmond Ball has argued that a nuclear war could not be sustained beyond the exchange of strategic warheads numbered not in the hundreds but in the tens (1981, 9). After a small number of exchanges no one would know what was going on or be able to maintain control. Yet nuclear weapons save us from our folly: fanciful strategies are irrelevant because no one will run the appalling risk of testing them.

Deterrence has been faulted for its lack of credibility, its dependence on perceptions, its destructive implications, and its inability to cover interests abroad. The trouble with deterrence, however, lies elsewhere: the trouble with deterrence is that it can be implemented cheaply. The claim that we need a seamless web of capabilities in order to deter does serve one purpose: it keeps military budgets wondrously high. Efforts to fashion a defensive and war-fighting strategy for NATO are pointless because deterrence prevails and futile because strategy cannot transcend the military conditions that nuclear weapons create.

Nuclear Arms and Disarmament

The probability of major war among states having nuclear weapons approaches zero. But the "real war" may, as William James claimed, lie in the preparation for waging it. The logic of deterrence, if followed, circumscribes the causes of "real wars" (1968, 23). Nuclear weapons make it possible for a state to limit the size of its strategic forces as long as other states are unable to achieve disarming first-strike capabilities by improving their forces.

Within very wide ranges, a nuclear balance is insensitive to variation in numbers and size of warheads. This has occasionally been seen by responsible officials. Harold Brown, when he was secretary of defense, said that purely deterrent forces "can be relatively modest, and their size can perhaps be made substantially, though not completely, insensitive to changes in the posture of an opponent." Somehow, he nevertheless managed to argue that we need "to design our forces on the basis of essential equivalents" (1979, 75–76). Typically, over the past three decades secretaries of defense have sought, albeit vainly, the superiority that would supposedly give us a war-fighting capability. But they have failed to explain what we can do with twelve thousand strategic nuclear warheads that we could not do with two thousand or an even smaller number. What difference does it make if we have two thousand strategic weapons and the Soviet Union has four thousand? We thought our deterrent did not deter very much and did not work with sufficient reliability just as we were reaching a peak of numerical superiority in the mid-1960s. Flexible response, with emphasis on conventional arms, was a policy produced in our era of nuclear plenty. "Superiority" and "parity" have had the same effect on our policy.

Many who urge us to build ever more strategic weapons in effect admit the military irrelevance of additional forces when, as so often, they give political rather than military reasons for doing so: spending less, it is said, would signal weakness of will. Yet militarily, only one perception counts, namely, the perception that a country has second-strike forces. Nuclear weapons make it possible for states to escape the dynamics of arms racing; yet the United States and the Soviet Union have multiplied their weaponry far beyond the requirements of deterrence. Each has obsessively measured its strategic forces against the other's. The arms competition between them has arisen from failure to appreciate the implications of nuclear weapons for military strategy and, no doubt, from internal military and political pressures in both countries.

Many of the obstacles to arms reduction among conventional powers disappear or dwindle among nuclear nations. For the former, the careful comparison of the quantities and qualities of forces is important. Because this is not so with nuclear weapons, the problem of verifying agreements largely disappears. Provisions for verification may be necessary in order to persuade the Senate to ratify an agreement, but the possibility of noncompliance is not very worrisome. Agreements that reduce one category of conventional weapons may shift competition to other types of weapons and lead to increases in their numbers and capabilities. Because with nuclear weapons sufficiency is easily defined, there is no military reason for reductions in some weapons to result in increases in others. Conventionally, multiparty agreements are hard to arrive at because each party has to consider how shifting alignments may alter the balance of forces if agreements are reached to reduce them. In a world of second-strike nuclear forces, alliances have little effect on the strategic balance. The Soviet Union's failure to insist that British, French, and Chinese forces be counted in strategic arms negotiations may reflect its appreciation of this point. Finally, conventional powers have to compare weapons of uncertain effectiveness. Arms agreements are difficult to reach because their provisions may bear directly on the prospects for victory or defeat. Because in a nuclear world peace is maintained by the presence of deterrent forces, strategic arms agreements do not have military but economic and political, significance. They can benefit countries economically and help to improve their relations.

A minority of U.S. military analysts have understood the folly of maintaining more nuclear weapons than deterrence requires. In the Soviet Union, Mikhail Gorbachev and some others have put forth the notion of "reasonable sufficiency," defined as having a strategic force roughly equal to ours and able to inflict unacceptable damage in retaliation. Edward Warner points out that some civilian analysts have gone further, "suggesting that as long as the USSR had a secure second-strike capability that could inflict unacceptable damage, it would not have to be concerned about maintaining approximate numerical parity with U.S. strategic nuclear forces" (1989, 21). If leaders in both countries come to accept the minority view—and also realize that a deterrent force greatly reduces conventional requirements on central fronts—both countries can enjoy security at much lower cost.

Strategic Defense

Strategic defenses would radically change the propositions advanced here.

The Strategic Defense Initiative, in Reagan's vision, was to provide an area defense that would protect the entire population of the United States. Strategic defenses were to pose an absolute defense against what have been absolute weapons, thus rendering them obsolete. The consequences that would follow from mounting such a defense boggle the mind. That a perfect defense against nuclear weapons could be deployed and sustained is inconceivable. This is so for two reasons: (1) it is impossible, and (2) if it were possible, it wouldn't last.

Nuclear weapons are small and light; they are easy to move, easy to hide, and easy to deliver in a variety of ways. Even an unimaginably perfect defense against ballistic missiles would fail to negate nuclear weapons. Such a defense would instead put a premium on the other side's ability to deliver nuclear weapons in different ways: firing missiles on depressed trajectories, carrying bombs in suitcases, placing nuclear warheads on freighters to be anchored in American harbors. Indeed, someone has suggested that the Soviet Union can always hide warheads in bales of marijuana, knowing we cannot keep them from crossing our borders. To have even modestly effective defenses we would, among other things, have to become a police state. We would have to go to extraordinary lengths to police our borders and exercise control within them. Presumably, the Soviet Union does these things better than we do. It is impossible to imagine that an area defense can be a success because there are so many ways to thwart it. In no way can we prevent the Soviet Union from exploding nuclear warheads on or in the United States if it is determined to do so.

Second, let us imagine for a moment that an airtight defense, however defined, is about to be deployed by one country or the other. The closer one country came to deploying such a defense, the harder the other would work to overcome it. When he was secretary of defense, Robert McNamara argued that the appropriate response to a Soviet defensive deployment would be to expand our deterrent force. More recently, Caspar Weinberger and Mikhail Gorbachev have made similar statements. Any country deploying a defense effective for a moment cannot expect it to remain so. The ease of delivering nuclear warheads and the destructiveness of small numbers of them make the durability of defenses highly suspect.

The logic of strategic defense is the logic of conventional weaponry. Conventional strategies pit weapons against weapons. That is exactly what a strategic defense would do, thereby recreating the temptations and instabilities that have plagued countries armed only with conventional weapons. If the United States and the Soviet Union deploy defensive systems, each will worry—no doubt excessively—about the balance of offensive and defensive capabilities. Each will fear that the other may score an offensive or defensive breakthrough. If one side should do so, it might be tempted to strike in order to exploit its temporary advantage. The dreaded specter of the hair trigger would reappear. Under such circumstances a defensive system would serve as the shield that makes the sword useful. An offensive-defensive race would introduce many uncertainties. A country enjoying a momentary defensive advantage would be tempted to strike in the forlorn hope that its defenses would be able to handle a ragged and reduced response to its first strike. Both countries would prepare to launch on warning while obsessively weighing the balance between offensive and defensive forces.

Finally, let us imagine what is most unimaginable of all—that both sides deploy defenses that are impregnable and durable. Such defenses would make the world safe for World War III—fought presumably in the manner of World War II but with conventional weapons of much greater destructive power.

Still, some have argued that even if some American cities remain vulnerable, defenses are very good for the cities they do cover. The claim is spurious. In response to the Soviet Union's deploying antiballistic missiles to protect Moscow, we multiplied the number of missiles aimed at that city. We expect to overcome their defenses and still deliver the "required" number of warheads. The result of defending cities may be that more warheads strike them. This is especially so because both they and we, working on worst-case assumptions, are likely to overestimate the number of missiles that the other country's system will be able to shoot down. Strategic defenses are likely to increase the damage done.

Most knowledgeable people believe that an almost leak-proof defense cannot be built. Many, however, believe that if improved hard-point defenses result from the SDI program, they will have justified its price. Defense of missiles and of command, control, and communications installations will strengthen deterrence, so the argument goes. That would be a solution, all right; but we lack a problem to go with it: deterrence is vibrantly healthy. If the Soviet Union believes that even one Trident submarine would survive its first strike, it will be deterred.[3] Since we do not need hard-point defenses, we should not buy them. The deployment of such defenses by one side would be seen by the other as the first step in deploying an area defense. Strategic considerations should dominate technical ones. In a nuclear world defensive systems are predictably destabilizing. It would be folly to move from a condition of stable deterrence to one of unstable defense.

Conclusion

Nuclear weapons dissuade states from going to war more surely than conventional weapons do. In a conventional world, states going to war can at once believe that they may win and that, should they lose, the price of defeat will be bearable. World Wars I and II called the latter belief into question before atomic bombs were ever dropped. If the United States and the Soviet Union were now armed only with conventional weapons, the lesson of those wars would be strongly remembered—especially by Russia, since she has suffered more in war than we have. If the atom had never been split, the United States and the Soviet Union would still have much to fear from each other. The stark opposition of countries of continental size armed with ever-more-destructive conventional weapons would strongly constrain them. Yet in a conventional world even forceful and tragic lessons have proved to be exceedingly difficult for states to learn. Recurrently in modern history one great power or another has looked as though it might become dangerously strong (Louis XIV's and Napolean's France, Wilhelm II's and Hitler's Germany). Each time, an opposing coalition formed, if belatedly, and turned the expansive state back. The lesson would seem to be clear: in international politics, success leads to failure. The excessive accumulation of power by one state or coalition of states elicits the opposition of others. The leaders of expansionist states have nevertheless been able to persuade themselves that skillful diplomacy and clever strategy might enable them to transcend the normal processes of balance-of-power politics. The Schlieffen Plan, for example, seemed to offer a strategy that would enable Germany to engage enemies on two fronts, serially: Germany would defeat France before Russia could mobilize fully and move westward in force. Later, Hitler, while denouncing the "boobs" of Wilhelmine Germany for getting themselves into a war on two fronts, reenacted their errors.

How can we perpetuate peace without

solving the problem of war? This is the question that states with nuclear weapons must constantly answer. Nuclear states continue to compete militarily. With each state tending to its security interests as best it can, war is constantly possible. Although the possibility of war remains, nuclear weapons have drastically reduced the probability of its being fought by the states that have them. Wars that might bring nuclear weapons into play have become extraordinarily hard to start. Over the centuries great powers have fought more wars, and lesser states have fought fewer: the frequency of war has correlated less closely with the attributes of states than with their international standing. Yet because of a profound change in military technology, waging war has more and more become the privilege of poor and weak states. Nuclear weapons have reversed the fates of strong and weak states. Never since the Treaty of Westphalia in 1648, which conventionally marks the beginning of modern history, have great powers enjoyed a longer period of peace than we have known since the Second World War. One can scarcely believe that the presence of nuclear weapons does not greatly help to explain this happy condition.

Notes

This essay was presented as the presidential address at the annual meeting of the American Political Science Association in Washington, 1988. David Schleicher's penetrating criticisms and constructive suggestions helped me greatly in writing it.

1. Nitze blandly adds that if we do launch on warning, "the estimates in the Soviet civil defense manuals are overoptimistic from the Soviet viewpoint" (1988, 357).

2. Quaint ideas die hard. In the fall of 1989 NATO resisted discussing naval disarmament with the Soviet Union because of the need for forces to guard the sea-lanes to Europe (Lewis 1988).

3. An Ohio-class Trident submarine carries twenty-four missiles, each with eight warheads.

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DOMESTIC OPPOSITION AND FOREIGN WAR

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Domestic opposition to violent, escalatory national policies during international crises has long been considered an important factor influencing the foreign policy behavior of nations. Yet the explicit theoretical linkages between domestic opposition and crisis choices have not been investigated. To provide these linkages, we set out an extensive form game of sequential decisions leading to the various consequences of crises together with their attendant costs and benefits. Our findings indicate that an antagonist's beliefs about domestic opposition are not particularly effective levers to manipulate in crises when a peaceful resolution is the goal.

A perennial puzzle in the study of international conflict concerns the impact of domestic political opposition on foreign policy. Although this issue has been widely discussed in the literature on war, we have scant theoretical understanding of the relationship between expected domestic opposition and the selection of foreign policy strategies. We do not know, for instance, whether the expectation of high domestic costs makes nations more or less likely to shun violent escalations of crises. Nor do we know how the uncertainties surrounding such domestic costs influence the responses of adversaries in crisis situations.

There is considerable disagreement among researchers about the role domestic factors play in foreign policy. Some believe that governments facing strong domestic opposition turn to foreign adventures or external scapegoats to manage domestic unrest, while others maintain that stiff domestic opposition provides an inviting environment for foreign aggressors (Blainey 1973; Hazelwood 1975; Kegley, Richardson, and Richter 1978; Rummel 1964; Tanter 1966; Wilkenfeld 1972). Still others claim that domestic political factors pale in comparison to the importance of systemic, structural factors in choosing between war and peace (Morgenthau 1973; Waltz 1979). We hope to shed some light on these competing perspectives, suggesting the conditions under which domestic considerations foster peace or encourage war.

Despite the empirical attention given to the linkage between domestic politics and foreign policy, we are not aware of a systematic theoretical analysis of this important issue. Our main purpose is, therefore, to contribute a step forward in developing such theoretical connections. We propose a sequential game of limited information that focuses on the role of domestic opposition to the use of force in shaping crises. We explore how uncertainties about domestic political opposition might be exploited to tip the outcomes of military crises toward peaceful, negotiated settlements and away from violent solutions and assess the harm or benefits such uncertainty fosters in crisis situations.

All violent foreign policies seem to elicit at least some political opposition. The United States' involvement in Vietnam, for instance, prompted substantial, highly visible domestic opposition. Other policies attract *nearly* unanimous support (as did the U.S. declaration of war against Japan in December 1941). But even as ostensibly clear a case as it seemed, the U.S. decision to retaliate for the attack on Pearl Harbor gave rise to a dissenting vote in the Congress, opposition from some quarters in the U.S. population, and scholarship arguing that a retaliatory strike was not needed (Russett 1972).

Beliefs about the level of domestic opposition to given foreign policies may profoundly influence the range of policy choices that a state has or the responses of adversaries to those choices. Former Secretary of Defense Caspar Weinberger, for instance, maintained that the United States had learned from experience that it was foolhardy to use troops in combat without substantial domestic political support. This belief probably was critical in shaping U.S. policies toward terrorism, support for the Contras, opposition to the Sandinistas, and approaches to the Middle East conflict during the Reagan years. And few would argue that domestic opposition was inconsequential in shaping U.S. and North Vietnamese policy during the Vietnam War. It is also likely that the Soviet decision to withdraw from Afghanistan was partially shaped by Russian domestic dissatisfaction with that war, just as the Russian decision to send troops into Afghanistan may have been encouraged by the Soviet belief that the United States could not constrain Soviet actions because of lingering, post-Vietnam opposition to engagements far from home.

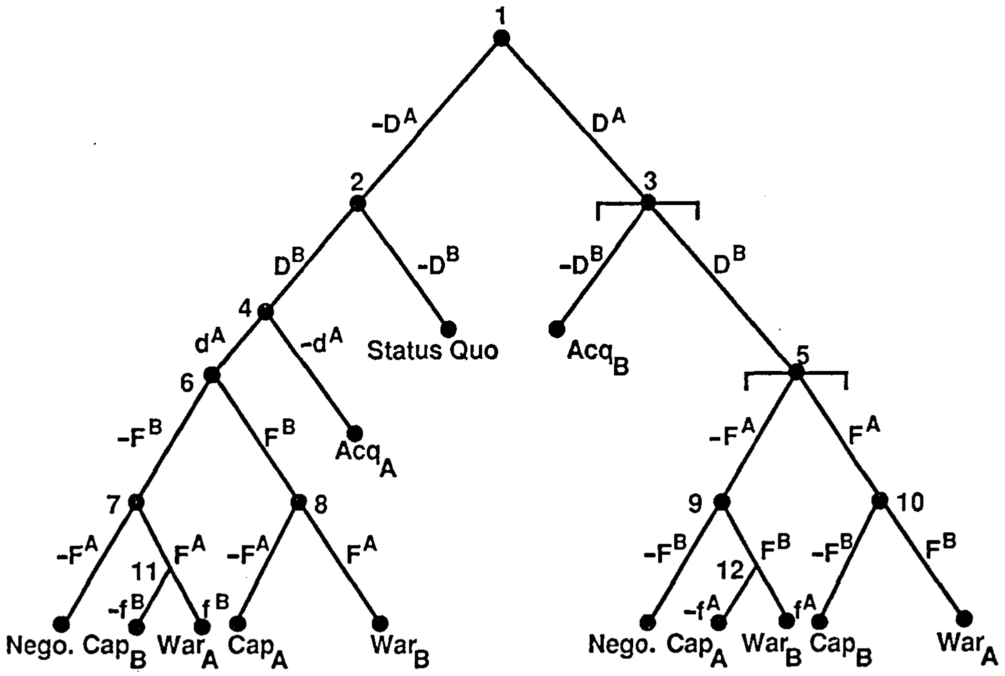
Discussions regarding domestic constraints on foreign policy have often revolved around whether "hawkish" or "dovish" approaches to international interactions are more likely to foster peace. Whether hawkishness or dovishness prompt peaceful (and acceptable) conflict resolution is a matter of great social (as well as analytic) concern. We begin our assessment of the relationship between these alternative approaches to foreign policy by describing an extensive form game of international interaction and then focusing on the particularly conflictual subgame that we call the *crisis game.* We focus specifically on domestic opposition to the use of force and how such opposition shapes the likely outcome of international disputes.

We use the term *hawkish nation* or *hawk* to refer to nations whose leaders expect to face, in the aggregate, relatively low domestic costs for using force against a foreign adversary. Nations are referred to as being *doves* or *dovish* if their leadership expects to face relatively high domestic costs for enforcing foreign policy demands through the use of violence. Of course, hawkishness and dovishness come in degrees that fall along a continuum; so we explore the threshold cost levels (and beliefs about those levels) that tip national choices from one set of strategies to another. Once the game has been described, we specify the conditions of uncertainty with which we will be concerned and then develop some general results regarding the linkage between domestic political opposition to conflict escalation and the prospects of such escalation.

The International Interaction Game

Although no two international interactions are exactly the same, there are still essential features in the development of relations between states. To describe these

Figure 1. International Interaction Game



essential features, the scaffolding from which foreign policies are built, we begin with the common assumption that nations can be treated as unitary actors. In the application of this assumption, we explicitly recognize that the fictional unitary actor is responsive to domestic political considerations as well as to calculations of support from its allies and support for its enemies from their allies (Altfeld and Bueno de Mesquita 1979; Iusi-Scarborough and Bueno de Mesquita 1988; Siverson and King 1980).

International interactions are assumed to begin with a move by nature—circumstances. This move by nature provides one or another state with an opportunity to take the initiative in shaping relations, an initiative that may govern the future development of events. The state "favored by circumstances" has available two initial moves: to make a demand (D) or not make a demand ($\sim D$) of another state. The demand may be about anything. We are not concerned here with the specific content of prospective disputes so much as with the *process* by which interactions evolve. Figure 1 depicts the extensive form of the international interaction game.

Once the actor (state A) has chosen its initial move, state B has the opportunity to select a course of action by making a demand or not. We say, for instance, that the status quo is maintained if A chooses not to make a demand ($\sim D^A$) and B chooses the same strategy ($\sim D^B$). If the sequence is D^A, $\sim D^B$, then B is said to acquiesce to A's demand, denoted by acq$_B$. Subscripts identify the decision maker *by* whom the specified action is taken. Should A forego the opportunity to initiate a demand (having calculated that its expected welfare is greater if it yields the initiative to *B*)—and *B* makes a demand ($\sim D^A$, D^B), *A* has the opportunity to acquiesce to *B*'s demand ($\sim D^A$, D^B, $\sim d^A \rightarrow \text{acq}_A$) or to make a counter demand ($\sim D^A$, D^B, $d^A \rightarrow$ military crisis). If the initial sequence of strategic choices were (D^A, D^B), there also is a crisis. Failure by either party to give in to the other's demand results in a crisis.

We characterize a military crisis as beginning at the moment when states *A* and *B* have each tabled demands (Lalman 1988) and there has not been acquiescence to either demand (Powell 1987). Up until the crisis stage there is no imminent threat of the use of force. A crisis is said to arise when a nation reaches the moment when it must choose between negotiating and escalating—between seeking to settle the dispute peacefully and resorting to arms to achieve its demands.

The subgame beginning at node 6 of the game depicted in Figure 1 is the same as the subgame beginning at node 5 except that *B*, rather than *A*, has the first move in the crisis. For the remainder of the discussion the focus is on the crisis subgame starting at node 5 or on the subgame that begins at node 3, in which *B* decides to acquiesce or make a counterdemand that leads to the crisis subgame. In these subgames the crisis is precipitated by *B*'s refusal to grant *A*'s demand. At node 5 *A* faces the choice of offering to negotiate by not initiating the use of force ($\sim F^A$) or of escalating the dispute by using force to back up its demand (F^A). If *A* elects to escalate the dispute by using force, *B* must choose between capitulating to *A*'s attack (F^A, $\sim F^B \rightarrow \text{cap}_B$) or striking back by using force itself (F^A, F^B). In the latter case the strategy sequence D^A, D^B, F^A, F^B results in a war initiated by state *A* (war_A). Should *A* elect to offer to negotiate ($\sim F^A$) at the outset of the crisis, *B*'s choices can lead to negotiations ($\sim F^A$, $\sim F^B$) or to escalation ($\sim F^A$, F^B). If *B* selects escalation, *A* must make a final choice: to capitulate to *B*'s enforced demand ($\sim F^A$, F^B, $\sim f^A \rightarrow \text{cap}_A$) or to retaliate ($\sim F^A$, F^B, f^A), resulting in a war initiated by *B* (war_B).

From Figure 1 it is evident that international interactions can culminate in eight different outcomes. The eight outcomes, with crisis outcomes listed in italics, are (1) status quo, (2) acquiescence by *A* to *B*'s demand, (3) acquiescence by *B* to *A*'s demand, (4) *negotiation,* (5) *capitulation by A,* (6) *capitulation by B,* (7) *war initiated by A,* and (8) *war initiated by B.*

The Crisis Game: Assumptions

The strategies players select are a function of the values they attach to alternative outcomes and the beliefs they hold regarding how their adversary will respond to their strategic decisions. Decision makers are assumed to respond to the circumstance they find themselves in by making the choice they believe maximizes their expected utility from that stage of the game onward. They cannot precommit themselves to one or another future course of action, but they can act in anticipation of their opponent's choices; that is, we assume subgame perfection (Selten 1975). Actors are thus assumed to be forward-looking rather than myopic (Brams and Wittman 1981). In this regard we explore only a single episode of international interaction, so that players are limited to looking forward down the path of a single conflict. We leave for future research an investigation of the implications of anticipated repeated crisis interactions.[1]

The value national leaders associate with the outcomes of crises (and therefore the set of admissible preferences over the outcomes) is determined in accordance with what seem to be innocuous restrictions. Six assumptions are required to establish the range of preferences of interest here:

1. Players (i, j) choose the strategy with the greatest expected utility given that they are restricted to subgame-perfect behavioral strategies.
2. The outcome of war is probabilistic rather than known, with P^i being player i's subjective probability of gaining its demand.
3. The initiator of force gains its demand with certainty if the adversary chooses to capitulate rather than retaliate. The capitulating state loses with certainty: cap_j implies $P^i = 1$ and $P^j = 0$, where P^i is the probability that actor i gains its demand.
4. All nations prefer to resolve crises through negotiations rather than through war: $U^i(N) > [U^i(\text{war}_i), U^i(\text{war}_j)]$.
5. Each outcome of the crisis game has potential benefits and/or costs associated with it. We decompose the costs into constituent parts such that $\alpha, \tau, \phi > 0$ and $\tau > \alpha, \gamma \bullet \alpha$ is the cost in lost life and property associated with fighting *away* from one's home territory; τ is the cost in life and property of fighting at home as the *target* of an attack; γ is the cost in life and property from a first strike to which the attacked party *gives in*; and ϕ is the domestic political cost (separate from lost life and property) associated with using *force*.[2] We assume that the utility associated with the costs is linear with the expected magnitude of the costs.[3]
6. The utility from gaining one's demands (Δ_i) is greater than the utility from losing by acceding to the adversary's demands (Δ_j): $U^i(\Delta_i) > U^i(\Delta_j) > 0$.

Assumption 1 stipulates that nations are led by rational, forward-looking, expected-utility-maximizing leaders.[4] Assumptions 2 and 3 are concerned with the probability of success, given alternative mixes of strategies for A and B. If either decision maker initiates a strategy of force, the adversary has the option to defend itself. We assume that a decision not to defend oneself in the face of a forceful challenge is a decision to accept defeat (assumption 3). This means that if the opponent does not respond with force, the challenger using force obtains with certainty whatever benefits were sought through its demand. For all events in the crisis subgame other than capitulation, the gains and losses are not determinate but are anticipated probabilistically.

For our model (as it seems to be for real world decision makers) the outcome of a war is not known with certainty. Though the expectations are based on subjective probabilities, a useful alternative interpretation is that the probabilities of the risks borne are based in accordance with the relative capabilities available to the adversaries, reflecting their gambling odds (Morrow 1985, 1989).[5] Nations A and B select their strategies on the basis of estimated values for these events.

In the case of negotiation the outcome is also a probabilistic expectation dependent on the relative capabilities of the antagonists and their respective coalitions of supporters (Banks 1990). We view negotiations as resulting in compromises that are somewhat akin to a weighted view of the common notion of "split the difference" (Rubinstein 1982). Assumption 4, in conjunction with assumption 5, stipulates that the expected outcome from negotiations, $U^i(N)$, is greater than the expected value of the outcome of war.[6] Thus,

$$U^A(N) > P^A U^A(\Delta_A) + (1 - P^A)U^A(\Delta_B) - \phi^A - \alpha^A$$

and

$$U^B(N) > P^B U^B(\Delta_B) + (1 - P^B)U^B(\Delta_A) - \phi^B - \alpha^B.$$

We attribute a first-strike advantage in a conflict by the assumption that $\tau > \alpha$ (assumption 5). A main advantage to the initiator is the enhanced ability to select the venue of fighting. The costs to a nation of being engaged in combat on its own territory are greater than the costs incurred when combat takes place on someone else's territory. The rationale is that combat on someone else's territory involves greater control over losses: the number of combatants and the amount of military material committed to the fight are at the discretion of the nation's political leadership. On the other hand, one expects to lose, in addition to military losses, civilian lives and nonmilitary property when fighting on one's own territory. These costs are not so easily controlled unless one capitulates (which is why $\gamma^i < \tau^i$).[7] Assumption 5 also recognizes that losses arise in relation to decisions to use force. We view these costs (ϕ) as being primarily political in nature. They entail domestic political opposition to using force in order to achieve policy goals. Immanuel Kant specifically addressed this cost when he wrote,

> If . . . the consent of the citizens is required in order to decide whether there should be war or not, nothing is more natural than that those who would have to decide to undergo all the deprivations of war will very much hesitate to start such an evil game. For the deprivations are many, such as fighting oneself, paying for the cost of war out of one's own possessions, and repairing the devastation which it costs, and to top all the evils there remains a burden of debts which embitters the peace and can never be paid off on account of approaching new wars" (quoted in Friedrich 1977, 438).

Examples of such costs arising out of domestic opposition in recent times include, for instance, demonstrations in the streets, the fear of electoral defeat or coup d'état, and difficulties in obtaining the funds with which to wage war. Congressional resistance to appropriating funds, for example, imposed direct political costs on U.S. policy makers associated with the Vietnam War during the late 1960s and early 1970s. Politicians take account of these costs just as surely as they are attentive to the expenditure of lives and national wealth. The very need to resort to force suggests a political failure by the national leadership, creating openings for opposition factions. The inability to settle international disputes through negotiations (the much less costly approach) reflects poorly on a nation's diplomacy. After all, recall that in the cases of interest here, negotiation is Pareto-superior to waging war (by assumption 4). Our thrust will be to develop a fuller understanding of how these domestic political costs shape decision making in crises.

The strategies available to nations *A* and *B* are the means by which they can realize gains or losses. Except for strategic combinations that eventuate in some form of submission, gains and losses are probabilistic such that decision makers must choose across lotteries. Table 1 gives the values associated with each event described at the terminal nodes of the subgames beginning at nodes 3 or 5 of Figure 1.

The Possibility of War With Perfect Information

PROPOSITION 1. *With perfect information, war can arise in the crisis game if and only if A's expectations over the outcomes are* $U^A(war_A) > U^A(cap_A) > U^A(war_B)$, *and B's expectations are* $U^B(cap_A) > U^B(N)$ *and* $U^B(war_A) > U^B(cap_B)$.

Proof. First we show that the conditions of the proposition are sufficient for the subgame-perfect equilibrium of war$_A$. Then we show that the conditions are necessary by demonstrating that relaxing any of the assumptions produces equilibria other than war.

Table 1. Expected Utilities Associated with Each Possible Outcome of the Crisis Subgame

	A's Expected Utility	B's Expected Utility
acq_B	$U^A(\Delta_A)$	$U^B(\Delta_A)$
nego	$P^A U^A(\Delta_A) + (1 - P^A) U^A(\Delta_B) \pm \epsilon^A$	$P^B U^B(\Delta_B) + (1 - P^B) U^B(\Delta_A) \pm \epsilon^B$
cap_A	$U^A(\Delta_B) - \gamma^A$	$U^B(\Delta_B) - \phi^B$
war_B	$P^A U^A(\Delta_A) + (1 - P^A) U^A(\Delta_B) - \phi^A - \tau^A$	$P^B U^B(\Delta_B) + (1 - P^B) U^B(\Delta_A) - \phi^B - \alpha^B$
cap_B	$U^A(\Delta_A) - \phi^A$	$U^B(\Delta_A) - \gamma^B$
war_A	$P^A U^A(\Delta_A) + (1 - P^A) U^A(\Delta_B) - \phi^A - \alpha^A$	$P^B U^B(\Delta_B) + (1 - P^B) U^B(\Delta_A) - \phi^B - \tau^B$

At node 12, player A chooses between the strategies $\sim f^A$ and f^A, $\sim f^A \rightarrow \text{cap}_A$ and $f^A \rightarrow \text{war}_B$. The proposition stipulates that for A, $U^A(\text{cap}_A) > U^A(\text{war}_B) \rightarrow \sim f^A$. Player B prefers cap_A to negotiations according to the proposition. Therefore, at node 9, F^B is preferred to $\sim F^B$. At node 10 B's strategy is F^B, implying war_A as the outcome because for B, $U^B(\text{war}_A) > U^B(\text{cap}_B)$. Therefore, at node 5 A's choice is between war_A and cap_A. Since, for A, $U^A(\text{war}_A) > U^A(\text{cap}_A)$, A chooses war. This establishes that the conditions stated in the proposition are sufficient for war.

The necessity of the conditions is shown by removing them one at a time. At node 12 if A had chosen war_B over cap_A, at node 9 B would have chosen $\sim F^B \rightarrow$ negotiation over F^B, since negotiation is strictly preferred to war by assumption 4. If the choice at node 12 were for A to capitulate to B and B preferred to negotiate with A rather than initiate the use of force, at node 9 B would have chosen $\sim F^B$. In either case A would choose negotiation rather than wage a war. At node 5, therefore, the choice would be $\sim F^A$, vitiating the possibility of war as the subgame-perfect equilibrium outcome. If the outcome at node 12 were to imply that A would capitulate to B so that A would be better off selecting F^A at node 5, war could only be averted if B preferred to capitulate to A rather than fight back, contradicting a condition stipulated in the proposition. Thus, we see that each condition is necessary if war is to be the subgame-perfect equilibrium of the crisis game and that they are sufficient. QED

Proposition 1 reveals some surprises about the risks of war among fully informed adversaries. A hawkish domestic opposition makes it easier, as noted earlier, for leaders to use violence to resolve foreign differences. However, because it is easier for such states to wage war, their adversaries are more likely to choose to negotiate with them. Dovish leaders are more likely to be exploited if they offer to negotiate. Consequently, although they find war less attractive than do hawks, they may be compelled to seize the first-strike advantage ($\tau > \alpha$) and wage war (Banks 1990; Van Evera 1984; Zagare 1987).

The realization that dovishness or hawkishness may yield seemingly counterintuitive and even perverse behavior leads us to wonder how uncertainties about domestic political constraints might influence the prospects for war or peace. Throughout the remainder of this investigation we focus on uncertainty about domestic opposition and its impact on the risk of war.

Uncertainty, Domestic Opposition, and the Risk of War

We begin by assessing the threshold of hawkishness or dovishness that drives crises toward war or peace. At the outset we assume that player *A*, the initiator of the crisis, knows everything that is relevant about nation *B*. Nation *B*'s leadership, however, is uncertain about the magnitude of domestic political opposition to the use of force in nation *A* (ϕ^A). This information is known to *A*'s leadership and can only be estimated by *B*'s leadership.

Once we have completed our analysis of the asymmetric information case that seems to favor *A*, we reverse the uncertainty, examining the expected strategic behavior of the players when *B* has private information regarding its domestic political costs, while *A*'s costs are known. Elsewhere (Bueno de Mesquita and Lalman n.d.) we examine two-way incomplete information about domestic political opposition to the use of force and its impact on the prospects for the negotiated settlement of disputes.

We choose to focus here on asymmetries in incomplete information for several reasons. The asymmetric cases are, of course, much less cumbersome to follow analytically than are the symmetric cases. Still, the asymmetric cases provide clues and insights into the prospective equilibria under two-way incomplete information. Furthermore, the prospect of an information asymmetry is quite real and interesting in its own right. The leader of nation *A*, as the initiator of a demand, probably knows more about its rival than the rival knows about it. After all, *A* selected *B* as the target of its demands. *B* had the situation foisted upon it. The United States, for instance, seems to have been rather uncertain about the staying power of the North Vietnamese during the Vietnam War, while the North appears always to have been confident that the United States lacked that staying power. As Ho Chi Minh tersely observed in 1965, "Americans don't like long, inconclusive wars. This is going to be a long, inconclusive war."[8]

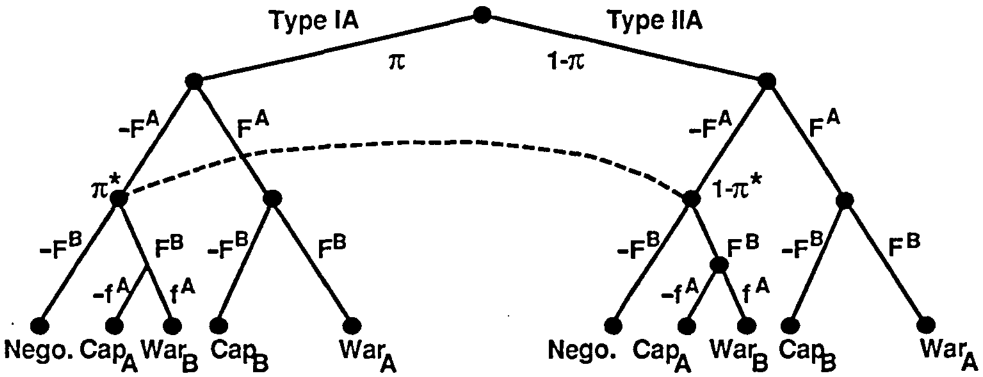
B, too, may sometimes possess an informational advantage. Although *B* is the target rather than the initiator of the crisis, *B* still had a prior opportunity to avoid the conflict and cut its losses by giving in to *A*'s initial demand. Thus, one can imagine circumstances in which *A*'s type is known to *B* as a result of *A*'s decision to make a demand, while *A* remains uncertain about the scope of *B*'s domestic opposition. Probably, the Japanese possessed just such an uncertainty about the United States when they decided to bomb Pearl Harbor. Although we speculate that this latter asymmetry occurs less frequently than the former, both are important in developing an understanding of interstate interactions and their tie to domestic affairs.

Uncertainty about the Initiator's Domestic Costs

Assume that the leadership of nation *B* is uncertain about the domestic political costs that its rival, *A*, can expect to incur by escalating a dispute to the point of violence. Because such uncertainty may profoundly shape behavior, we are interested in exploring the equilibrium conditions brought about by such uncertainty in cases where there is a nonzero belief that the conditions of proposition 1 hold.

Assume that *B* believes the domestic political costs of conflict escalation borne by the leaders in *A* is either ϕ or $\delta\phi$, where $0 < \delta < 1$. Since $\delta\phi < \phi$, it follows that nations of the sort that bear $\delta\phi$ find it politically easier to pursue their demands (with force if necessary) than do nations bearing the higher cost, ϕ; that is, states facing a domestic political cost of $\delta\phi$ are relatively hawkish, while those facing a cost of ϕ are relatively dovish. Paradoxically, under complete information, those states facing high-enough domestic political costs to prefer to capitulate rather than retaliate if attacked are states

Figure 2. Crisis Game with Uncertainty about A's Domestic Costs



that in equilibrium are more likely to choose to *initiate* war. Those anticipating sufficiently little domestic political opposition from using force, conversely, are more likely to end up reaching a negotiated settlement of their disputes. This is so because the credibility of the threat to retaliate if attacked is diminished if the political costs associated with further escalation are high. Highly credible threats encourage negotiated settlements. As the prospective threat of retaliation is diminished, states with high domestic political costs for escalating crises are pushed either to accept capitulation or to strike preemptively by initiating a war (Schelling 1966; Van Evera 1984).

The above discussion (a direct implication of proposition 1) suggests that in the absence of uncertainty doves are likely to escalate their disputes with hawks, making the doves act like the aggressors. But how might uncertainty alter the outcome of crises? In particular, how might uncertainty affect strategic behavior when that uncertainty influences beliefs about how nation *A* would respond to nation *B*'s use of force? Whether nation *A* is the type that would capitulate or retaliate to the use of force by nation *B* is the critical uncertainty.

If *A* is the type that would capitulate, *B* has a strong incentive to exploit offers by *A* to negotiate, striking against *A* and forcing its capitulation. *A*, anticipating this exploitation, has an incentive to attack *B* first, given the restriction from proposition 1 that for *A*, $U^A(\text{war}_A) > U^A(\text{cap}_A)$. If *A* is the type that would retaliate if attacked, *B* has no incentive to try to exploit an offer by *A* to negotiate. *B*, after all, prefers to negotiate rather than fight a war (assumption 4). Thus, the course of action that *B* selects if *A* offers to negotiate is critically dependent on whether *B* believes *A* will capitulate or retaliate.

We fix δ to create this uncertainty. Thus,

$$\delta\phi^A < \{P^A[U^A(\Delta_A) - U^A(\Delta_B)] - \tau^A + \gamma^A\} < \phi^A.$$

These two inequalities indicate that for type 1 *A* (the actor whose domestic political costs are discounted by δ), war_B is preferred to cap_A, while for type 2 *A*, $U^A(\text{cap}_A) > U^A(\text{war}_B)$. Type 1, then, is expected to retaliate if attacked, while type 2 concedes an advantage to *B* by not retaliating if attacked.

Since we have assumed that negotiations are strictly preferred to waging war, it is appropriate to define that relationship more closely. We constrain the value of negotiations such that

$$U^i(N) = P^iU^i(\Delta_i) + (1 - P^i)U^i(\Delta_j) \pm \epsilon^i > P^iU^i(\Delta_i) + (1 - P^i)U^i(\Delta_j) - \delta\phi^i - \alpha^i,$$

so that even the cheapest form of war is not as attractive as negotiating a settlement to a dispute.

Figure 2 depicts the decision problem confronting nations *A* and *B*. In the event that *A* offers to negotiate, nation *B* does not know if it would be better off negotiating (as it would if *A* is of type 1) or enforcing its demands and compelling *A* to capitulate (which would occur if *A* is of type 2). It is common knowledge at the outset of the game that *B* believes *A* is type 1 with probability π. π^* represents *B*'s beliefs about *A*'s type as updated in accordance with Bayes's Rule at the information set denoted by the broken line in Figure 2. We stipulate, in accordance with proposition 1, that *B* prefers cap_A to negotiation and that if attacked, *B* prefers to retaliate rather than capitulate to *A*, for *B* $U^B(\text{war}_A) > U^B(\text{cap}_B)$. For simplicity in notation, we write the utility *B* attaches to its expectations over these outcomes as follows:

$$U^B(\text{cap}_A) = a; \; U^B(N) = b;$$
$$U^B(\text{war}_B) = c; \; U^B(\text{war}_A) = d;$$
$$U^B(\text{cap}_B) = e;$$

where $a > b > c > d > e$. Since we are interested in the relationship between beliefs about *A*'s political opposition and the prospects for war or peace, we have selected utilities for player *B* consistent with proposition 1 so that war is possible as the outcome of a sequential equilibrium of the crisis (Cho and Kreps 1987; Kreps and Wilson 1982).[9] The question, then, is How do *B*'s beliefs about *A*'s domestic political opposition shape whether the equilibrium outcome of the dispute is war or negotiation?

If *B* reaches the information set in Figure 2, what strategy will be chosen, F^B or $\sim F^B$? If the leadership in *B* believes nation *B* will be better off negotiating than risking a retaliatory strike by *A* in the event *B* uses force, *B* will elect to negotiate. This is equivalent to

$$U^B(N) > \pi^*(c) + (1 - \pi^*)(a). \qquad (1)$$

Relationship 1 implies that *B*'s strategy will be $\sim F^B$. If π^* (the updated belief about the likelihood that nation *A* is type 1) is large enough, *B* will choose negotiation over the risky lottery. A simple backward induction will convince the reader that if π^* is large enough, nation *A*, whether it is type 1 or type 2, will offer to negotiate rather than use force. Provided the initial belief about *A*'s type, π, is big enough, type 2 will have no incentive to call attention to its high domestic costs by behaving differently from type 1; that is, if $\pi > (a - b)/(a - c)$, *B* is unable to separate between type 1 and type 2 *A*s on the basis of observed behavior. Consequently, *B* has no basis to alter the initial belief about *A* when the opportunity to update presents itself at the information set in the game. Thus, one pooling, sequential equilibrium of the game is

$$\sim F^A, \sim F^A, \sim F^B; \pi^* > (a - b)/(a - c).$$

The equilibrium is read as, Type 1 *A* offers to negotiate; type 2 *A* offers to negotiate; *B* at this information set chooses to negotiate rather than fight; and at the information set *B* believes that *A* is type 1 with a probability greater than the fraction $(a - b)/(a - c)$. The equilibrium notation lists the strategy of type 1 *A*, type 2 *A*, *B*, and the belief at the information set.

This first equilibrium reveals that if *B* is sufficiently confident that nation *A* faces little domestic opposition to using force, *B* has no wish to challenge *A* during the crisis. Instead, *B*, believing that the adversary is relatively hawkish, will negotiate.

This implies that dovish states (type 2) would find it beneficial to bluff hawkishness. Whether, through bluffing, type 2 nations can send a deceptive message that induces learning in nation *B* that raises *B*'s belief that *A* will retaliate if attacked is an issue we will return to shortly. When we do, it will become evident that *A* cannot make negotiated resolutions of crises more likely by engaging in a successful strategy to alter *B*'s beliefs about *A*'s domestic political opposition.

A second equilibrium, like the first, is also a pooling equilibrium in which types 1 and 2 respond the same way to the anticipated behavior of player *B*. But this equilibrium, in which *B* has greater doubts about nation *A*'s being a hawkish type 1 state, supports the expectation of war.

If $U^B(N) < \pi^*(c) + (1 - \pi^*)(a)$, *B*'s strategy will be F^B. Type 1 *A* and type 2 *A* will each, for somewhat different reasons, choose to initiate a war rather than offer to negotiate. Type 2 fights because if it offered to negotiate it would wind up being exploited by nation *B*. Since type 2 prefers war_A to cap_A, *A* uses force. Type 1 *A* would retaliate if attacked. Thus, for type 1 *A* the choice is between war_B or war_A. Since the latter is preferable (because $\tau > \alpha$), type 1 will choose to fight rather than offer to negotiate. The second possible equilibrium is

$$F^A, F^A, F^B; \pi^* < (a - b)/(a - c).$$

In this equilibrium, player *B* never gets to the information set depicted in Figure 2. Rather, both types of *A* anticipate that *B* will elect to use force if they offer to negotiate and, as noted, since both types are better off acting preemptively than they are turning over the initiative to *B*, either type would attack *B*, given the initial beliefs. In these cases the nations end up waging a war when both would have been better off negotiating.

It is clear from the two equilibria just reported that if nation *A* is of type 2, it has an incentive to try to encourage the belief that it is the more hawkish type 1. If nation *A* can raise nation *B*'s initial belief, $\pi < (a - b)/(a - c)$, so that its updated belief $\pi^* \geq (a - b)/(a - c)$, *A* can reduce the risk of war and enhance the prospects of resolving its crises through negotiations. Dovish *A*s, wishing to avert war, do have an incentive to act hawkish.

Given the relatively high domestic cost of such behavior and the consequences for having its bluff called, nations of type 2 are not free to engage in unlimited hawkish behavior. The leadership is constrained by the internal costs and by the rational expectation that *B* will test the waters, challenging them from time to time to see if they really are hawks or if they are bluffing. Thus, type 2 *A*s, in trying to teach *B* that they are rather hawkish, are limited by their own domestic constraints in the mixed strategy they can follow. We calculate the mixed strategy sequential equilibrium to ascertain the extent to which dovish nations can raise *B*'s expectation that nation *A* is a hawkish type 1 state by strategically manipulating *B*'s beliefs.

A's objective is to raise π^* so that *B*'s belief at the information set equals at least $(a - b)/(a - c)$. To do so, *A*s of type 2 must act as if they are more willing to meet force with force than *B* otherwise would have expected. By picking the right mix of behaviors, *A* hopes to look more like type 1 to *B*, thereby deceiving *B*. By deceiving *B* in this way, at least some violent conflicts would be replaced by negotiations. We let the probability that *A* selects the strategy $\sim F^A = \lambda^A$. λ^A is picked such that the updated value of $\pi^* = (a - b)/(a - c)$. The value of λ^A depends upon *A*'s expectations regarding *B*'s reaction to *A*'s strategy. *A* wants to be convincing in its efforts to appear hawkish.

B understands that *A* may be bluffing hawkishness. Consequently, *A* can anticipate that B will from time to time negotiate when it reaches the information set and from time to time B will challenge A to see if A capitulates or retaliates. In this way B forces A to be somewhat cautious in its actions. In particular, B picks a mix of responses when A offers to negotiate, such that A's hesitation to risk exploitation is bounded. This boundary is created by making A indifferent between initiating the use of force on its own (F^A) and facing the risk of a called bluff from B ($\sim F^A$, $\varrho^B \sim F^B$, $[1 - \varrho^B]F^B$). The probability with which B will not call A's bluff is equal to ϱ^B, such that

$$\begin{aligned}\varrho^B[U^A(N)] &+ (1 - \varrho^B)[U^A(\Delta_B) - \gamma^A] \\ &= P^A U^A(\Delta_A) + (1 - P^A)U^A(\Delta_B) \\ &- \alpha^A - \phi^A. \qquad (2)\end{aligned}$$

Relationship 2 is equivalent to the statement that

$$\varrho^B = \frac{P^A[U^A(\Delta_A) - U^A(\Delta_B)] - \alpha^A - \phi^A + \gamma^A}{P^A[U^A(\Delta_A) - U^A(\Delta_B)] + \gamma^A \pm \epsilon^A}. \qquad (3)$$

Type 2 A in picking a mix of strategies ($\lambda^A \sim F^A$, $[1 - \lambda^A]F^A$) and B in selecting a response must both consider how type 1 would respond to B's anticipated strategy. Type 2 A is cognizant of this factor because it is trying to "look like" type 1. B considers this factor because it is inefficient to challenge A too often, given the possibility that A is the hawkish type 1 and will retaliate if attacked. If B's mixed strategy challenges A too often and A is type 1, B will *always* face a preemptive attack. Thus, in order for A's and B's mixed strategies to be effective, it must be true that if A is of type 1; then,

$$\varrho^B \geq \frac{\tau^A - \alpha^A}{\tau^A + \delta\phi^A \pm \epsilon^A}. \qquad (4)$$

Relationship 4 tells us that if the probability that B will not try to exploit A by using force is large enough, actors of type 1 will always offer to negotiate. This feature is critical if type 2 As are to benefit from efforts to deceive B into thinking they are type 1. Given ϱ^B large enough that type 1 will always elect $\sim F^A$ rather than F^A, the probability with which type 2 chooses $\sim F^A$ is

$$\lambda^A = \frac{\pi(b - c)}{(1 - \pi)(a - b)}.$$

The mixed strategy sequential equilibrium *appears to be*

$$\begin{aligned}&\sim F^A,\ [\lambda^A \sim F^A,\ (1 - \lambda^A)F^A],\\ &[\varrho^B(\sim F^B),\ (1 - \varrho^B)F^B];\\ &\pi^* = (a - b)/(a - c).\end{aligned}$$

This equilibrium would appear to suggest that type 2 As have an opportunity to engage in strategic deception that will make their crises more likely to be resolved peacefully than would otherwise be the case. However, we shall prove that such a strategy is *not* available. We state this as a theorem and offer the proof below:

THEOREM 1: *If type 2 A nations hold the preferences delineated in proposition 1, and type 1 A prefers war$_B$ to cap$_A$ so that* $\delta\phi^A < \{P^A[U^A(\Delta_A) - U^A(\Delta_B)] - \tau^A + \gamma^A\}$, *there is no value of* δ $(0 < \delta < 1)$ *such that*

$$\varrho^B \geq (\tau^A - \alpha^A)/(\tau^A + \delta\phi^A \pm \epsilon^A).$$

In other words, type 2 nations cannot engage in strategic deception that sufficiently revises B's belief that A is a type 1 nation to induce negotiations when war would otherwise have occurred.

Proof. We show that there is no value for δ $(0 < \delta < 1)$ consistent with the preferences stipulated for either type 1 or type 2 nations.

Substituting the values for ϱ^B from relationships 3 and 4 we find that the mixed strategy equilibrium requires that

$$\frac{\tau^A - \alpha^A}{\tau^A + \delta\phi^A \pm \epsilon^A} \leq \frac{P^A[U^A(\Delta_A) - U^A(\Delta_B) - \alpha^A - \phi^A + \gamma^A}{P^A[U^A(\Delta_A) - U^A(\Delta_B)] + \gamma^A \pm \epsilon^A}. \quad (5)$$

We begin by identifying the conditions under which the inequality in relationship 5 can hold.

First, suppose the numerator on the left side of relationship 5 is less than or equal to the numerator on the right side. Then

$$\tau^A + \phi^A - \gamma^A \leq P^A[U^A(\Delta_A) - U^A(\Delta_B)]. \quad (6)$$

The assumption that type 2 A prefers cap_A to war_B means that

$$\tau^A + \phi^A - \gamma^A > P^A[U^A(\Delta_A) - U^A(\Delta_B)].$$

Therefore, relationship 6 cannot be true. The numerator on the left *must* be bigger than the numerator on the right side of relationship 5.

Second, suppose the numerator and denominator on the left side of the inequality in relationship 5 are greater than the numerator and denominator respectively on the right side. Then

$$\tau^A + \delta\phi^A - \gamma^A > P^A[U^A(\Delta_A) - U^A(\Delta_B)].$$

Proposition 1 specifies that type 1 A prefers war_B to cap_A. This preference is equivalent to the statement that

$$\tau^A + \delta\phi^A - \gamma^A < P^A[U^A(\Delta_A) - U^A(\Delta_B)]. \quad (7)$$

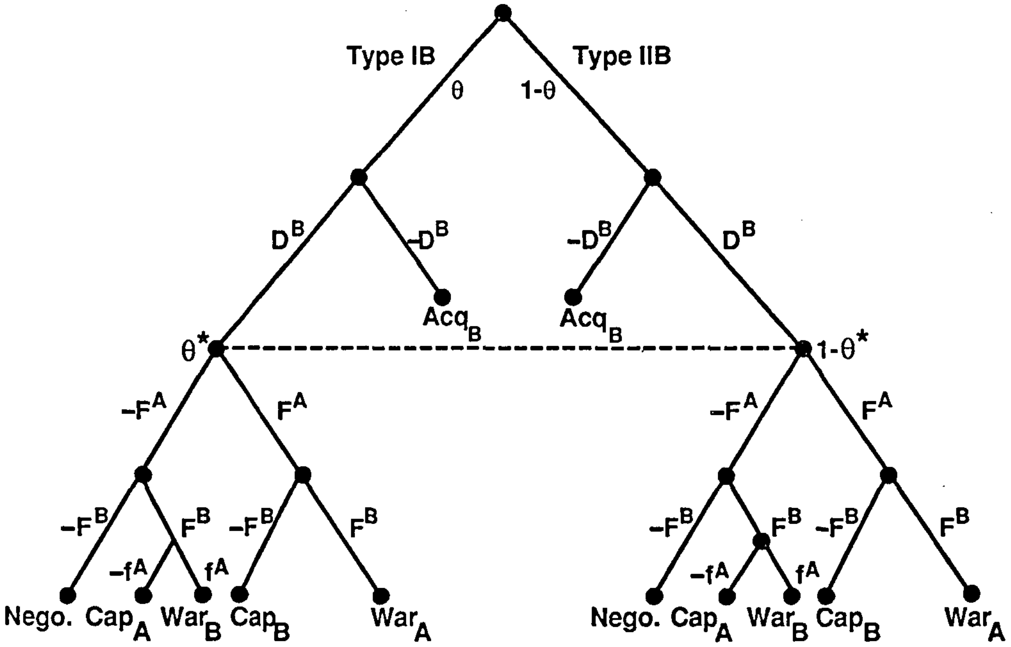
Relationship 7 contradicts the inequality from expression (5). No value of δ can support the equilibrium in this case.

Third, the remaining possible mixes of numerator and denominator inequalities all contradict the inequality in relationship 5. Thus, if the left denominator equals the right denominator, the left numerator cannot be larger than the right numerator and maintain the inequality from relationship 5. Likewise, if the left denominator is smaller than the right denominator in relationship 5, then the left numerator cannot be greater than the right numerator and still maintain the direction of the inequality in relationship 5. QED

Theorem 1 reveals that given the crisis subgame, no level of deception about domestic opposition to threats of escalation can be used to communicate a revised image that will influence an adversary's course of action. Thus, uncertainty over whether a rival is greatly opposed or only mildly opposed at home when it threatens to escalate a dispute does not deflect its opponent from its intended course of action. So long as the adversary believes the opponent has a relatively high probability of being type 1, $\pi^* \geq (a - b)/(a - c)$, it will negotiate rather than fight. Regardless of whether the initiator offers to negotiate or not, an adversary will use force so long as it holds a sufficiently high belief that the initiator of the dispute is of type 2, $\pi^* < (a - b)/(a - c)$. No amount of effort by the initiator to alter the opponent's beliefs regarding domestic costs will suffice to induce negotiations where force might otherwise have been used. Such efforts are doomed to being ineffective because they necessarily lack credibility.

The problem for type 2 nations is that once the prior belief is no longer greater than $(a - b)/(a - c)$, any bluff of toughness is too risky. By bluffing in such circumstances, the probability of being exploited by nation B is just too great. If A really were a type 1 hawkish state, then, under the stipulated conditions, it would fight for sure. If B witnesses an effort by A to negotiate, B *knows* that A is type 2. Type 2 can only raise B's initial beliefs by

Figure 3. Crisis Game with Uncertainty about B's Domestic Costs



acting "out of character" in this circumstance. Acting out of character, regrettably for type 2, means fighting rather than offering to negotiate. Consequently, dovish *A*s cannot encourage negotiations by reeducating their rivals to believe they are hawkish under the conditions set out in theorem 1. Rather, type 2 nations, with relatively dovish populations, face a greater risk of war—wars they themselves start—than do hawkish nations.

Uncertainty about the Target's Domestic Costs

We now examine the circumstances under which state *A*, facing the choice between using force or offering to negotiate, is uncertain about the reaction of the relevant domestic population in the rival state. Will its rival find that fighting back involves minimal political costs, or will it confront strong opposition from within? How will *A*'s uncertainty about rival state *B* influence the equilibrium of the crisis game?

As we are particularly interested here in strategies to avoid war during crises, we restrict our attention to the cases in which state *A*'s preferences are consistent with the conditions in proposition 1. In particular, let *A*'s utility for each outcome in Figure 3, which depicts the game between *A* and *B* when there is uncertainty about *B*, equal $U^A(\text{acq}_B) = a$, $U^A(N) = b$, $U^A(\text{cap}_B) = c$, $U^A(\text{war}_A) = d$, $U^A(\text{cap}_A) = e$, and $U^A(\text{war}_B) = f$, where $a > b > c > d > e > f$.[10]

Nation *A* is uncertain of *B*'s preferences over outcomes. Suppose that type 1 *B* has a dovish population that therefore imposes relatively high domestic costs on the government for using violence. Suppose *B*s of type 2 face relatively little domestic political opposition to forceful foreign policies, so that $\delta\phi$ is the domestic cost for type 2 and ϕ is the cost for type 1. Let *A*'s prior belief that B is of type 1 equal θ (so that $1 - \theta$ is the probability that B is of type 2) and A's updated belief at A's information set (the dotted line in Figure 3) equal θ^*.

THEOREM 2. *If type 2 B bears* $\delta\theta^B$ *and type 1 B incurs* θ^B, *with* $0 < \delta < 1$ *and if for both types of B* $U^B(war_A) > U^B(cap_B)$, *there is no level of strategic deception that can sufficiently revise A's belief about B's type to induce negotiations when war or acquiescence by B to A would otherwise occur.*

Proof. To prove the theorem we examine all cases that are consistent with our initial assumptions and the restriction that $0 < \delta < 1$. We note in this regard that it is not possible for type 1 B to prefer war over acquiescence to A if type 2 B prefers to acquiesce rather than retaliate against an attack by A. Given the payoffs we have stipulated, such an ordering would require that $\delta\phi > \phi$ for B, which violates the restriction that $\delta < 1$.

Case 1. Let $U^B(\Delta_B) - \phi^B$ and $U^B(\Delta_B) - \delta\phi^B > U^B(N)$ so that $U^B(\text{cap}_A) > U^B(N)$ for all B. Then, $D^B \rightarrow P^BU^B(\Delta_B) + (1 - P^B)U^B(\Delta_A) - \phi^B - \tau^B$ (i.e., war$_A$) and $\sim D^B \rightarrow U^B(\Delta_A)$ (i.e., acquiescence by B to A). Then the equilibrium depends only on whether B prefers the outcome war$_A$ to acquiescence to A. If type 2 prefers war while type 1 prefers to acquiesce, it is self-evident that the only possible equilibrium is

$$\sim D^B, D^B, F^A, F^B, F^B; \theta^* = 0. \qquad (8)$$

The equilibrium described by relationship 8 provides no opportunity to reach a negotiated settlement of the dispute even though all uncertainty about B's type is removed by the time A reaches its information set.

Case 2. If $U^B(N) > U^B(\Delta_B) - \delta\phi^B$ and therefore $U^B(N) > U^B(\Delta_B) - \phi^B$, uncertainties about domestic costs do not influence the equilibrium of the game. $D^B \rightarrow \sim F^A$ (by assumption 4) and $\sim F^A \rightarrow$ negotiation in this case. Then, $D^B \rightarrow U^B(N)$ and $\sim D^B \rightarrow U^B(\Delta_A)$, $U^B(N) > U^B(\Delta_A)$. Therefore, regardless of $\delta\phi$ and ϕ, the equilibrium is

$$D^B, D^B, \sim F^A, \sim F^B, \sim F^B; \theta^* \geq 0.$$

Case 3. Let $[U^B(\Delta_B) - \delta\phi^B] > [P^BU^B(\Delta_B) + (1 - P^B)U^B(\Delta_A) \pm \epsilon^B] > [U^B(\Delta_B) - \phi^B]$. Let $[P^BU^B(\Delta_B) + (1 - P^B)U^B(\Delta_A) - \phi^B - \tau^B] > U^B(\Delta_A)$. Then $D^B > \sim D^B$ for all B regardless of A's decision to use force or to offer to negotiate. Thus, A cannot update its belief about B's type at A's information set. By assumption 1 then, the equilibria under these conditions are

$$D^B, D^B, \sim F^A, \sim F^B, F^B;$$
$$\theta^* > (d - e)/(b - e)$$

and

$$D^B, D^B, F^A, F^B, F^B;$$
$$\theta^* < (d - e)/(b - e).$$

A's action is dependent on its prior belief (θ) about B's type, but is not dependent on the action B takes. In this case B can neither increase the probability of negotiations nor decrease the risk of war by exploiting uncertainty about the magnitude of its domestic opposition to the use of force.

Case 4. Assume the same conditions as in case 3, but let $U^B(\text{acq}_B) > U^B(\text{war}_A)$ for all B. $D^B \rightarrow \sim F^A$ if $\theta > (d - e)/(b - e)$ and $D^B \rightarrow F^A$ if $\theta < (d - e)/(b - e)$. Since $U^B(\text{acq}_B) > U^B(\text{war}_A)$ for all B in this case, if $\theta < (d - e)/(b - e)$, $\sim D^B > D^B$ for both type 1 and type 2 B. A learns nothing at the information set and the posterior probability is unaltered.

The subgame-perfect sequential equilibria in this case are

$$D^B, D^B, \sim F^A, \sim F^B, F^B;$$
$$\theta^* > (d - e)/(b - e)$$

and

$$\sim D^B, \sim D^B, F^A, F^B, F^B;$$
$$\theta^* < (d - e)/(b - e).$$

As with case 3, there are no strategies available to player B by which it can manipulate A's beliefs about domestic reactions in B to B's use of force.

Case 5. Let $[U^B(\Delta_B) - \delta\phi^B] > [P^B U^B(\Delta_B) + (1 - P^B)U^B(\Delta_A) \pm \epsilon^B] > [U^B(\Delta_B) - \phi^B]$; $[P^B U^B(\Delta_B) + (1 - P^B)U^B(\Delta_A) - \delta\phi^B - \tau^B] > U^B(\Delta_A) > [P^B U^B(\Delta_B) + (1 - P^B)U^B(\Delta_A) - \phi^B - \tau^B]$. A will choose $\sim F^A$ if $\theta^* > (d - e)/(b - e)$ and player A will select F^A if $\theta^* < (d - e)/(b - e)$. If $\theta^* > (d - e)/(b - e)$, type 1 B will negotiate with A because $P^B U^B(\Delta_B) + (1 - P^B)U^B(\Delta_A) \pm \epsilon^B > U^B(\Delta_B) - \phi^B$ while type 2 B will exploit A, leading to a capitulation by A to B's enforced demand. Thus, only at the *end of the game* will A learn whether B is type 1 or type 2. But at A's information set it will not yet have obtained new information (beyond its prior belief, θ) about B's type.[11] The equilibrium in the former case is

$$D^B, D^B, \sim F^A, \sim F^B, F^B;$$
$$\theta^* > (d - e)/(b - e). \tag{9}$$

In the event that the initial belief $\theta < (d - e)/(b - e)$, type 1 B and type 2 B will behave differently prior to A's choice at its information set. Type 1 decision makers are better off acquiescing to A's demands than fighting a war with A, while type 2 nations, with their lower domestic costs for resorting to force, are better off fighting than they are giving in at the outset. Type 1 B will choose $\sim D^B$ and type 2 B will choose D^B if the prior belief $\theta < (d - e)/(b - e)$. Given such a prior belief, the equilibrium is

$$\sim D^B, D^B, F^A, F^B, F^B; \theta^* = 0. \tag{10}$$

Can type 1 B improve its prospects by pursuing a mixed strategy, sometimes making a demand and sometimes not making a demand when $\theta < (d - e)/(b - e)$? The answer is *no*! To do at least as well as in relationship 10, type 1 B might try a mixed strategy designed to raise $\theta^* = 0$ to $\theta^* = (d - e)/(b - e)$.

Let ϱ^B be the probability with which type 1 B makes a demand when $\theta < (d - e)/(b - e)$. Let λ^A be the probability with which A offers to negotiate. A selects λ^A so that it is indifferent between F^A (in which case A fights a war for sure) and $\sim F^A$ (in which case, if B is type 1 there will be negotiations and if B is type 2 A will be forced to capitulate), given that B selects D^B with probability ϱ^B. This condition is satisfied when

$$\lambda^A = \frac{\tau^B + \phi^B - P^B[U^B(\Delta_B) - U^B(\Delta_A)]}{\tau^B + \phi^B \pm \epsilon^B}. \tag{11}$$

In order for λ^A to satisfy relationship 11, A must update θ at the information set so that $\theta^* = (d - e)/(b - e)$. Following Bayes's Theorem,

$$\varrho^B = \frac{(1 - \theta)(d - e)}{\theta\,(b - e)}. \tag{12}$$

If the equality in relationship 12 is satisfied, the equilibrium would be

$$[\varrho^B D^B, (1 - \varrho^B)\sim D^B], D^B,$$
$$[\lambda^A \sim F^A, (1 - \lambda^A)F^A];$$
$$\theta^* = (d - e)/(b - e),$$

so that type 1 B, rather than acquiescing to A, would with probability $\varrho^B\lambda^A$ end up negotiating a resolution of the dispute that is better for itself, but at the price of waging a costly war with probability $\varrho^B(1 - \lambda^A)$. The possibility of inducing such negotiated solutions through the strategy of picking D^B with probability ϱ^B would contradict the theorem. However, since $0 < \varrho^B < 1$, $\theta(b - e) > (1 - \theta)(d - e)$. This can only be true if $\theta > (d - e)/(b - e)$. But type 1 B would not select $\sim D^B$ when $\theta > (d - e)/(b - e)$ since the equilibrium in relationship 9 is superior for type 1 B to the proposed mixed strategy equilibrium. Therefore, a mixed strategy equilibrium is *not* supportable as a means to increase the prospects of attaining a negotiated settlement.

This exhausts the feasible cases. Therefore, in no case can B alter A's initial belief about B's type to induce negotiations and avoid war. QED

Theorem 2 mirrors for nation B the result found in theorem 1 for nation A. Thus, neither party to a dispute of the sort modeled here can communicate a belief about its domestic opposition's hawkishness or dovishness that will alter the course of a crisis, steering events away from war and toward a negotiated settlement. Of course, successful bluffing can occur in the sense that A's initial belief may be wrong about B (or B's initial belief may be wrong about A). In this regard, it is evident from the equilibria of the game that uncertainty about domestic opposition does alter the prospects of war or peace. But neither party can signal new information that alters the behavior of the other party from what that behavior is expected to be given the initial beliefs about one another. Our analysis of the equilibria in this model show that uncertainty sometimes can foster peaceful resolutions of disputes, but that uncertainty cannot itself be subjected to artful, strategic manipulation to make peace more likely.

The results of theorems 1 and 2 place the Kantian hypothesis cited earlier in an altered perspective. If B believes A faces serious domestic opposition to the use of force and A knows that B holds that belief, then, contrary to Kant, A is inclined to seize any advantage that a first-strike gives it ($\tau > \alpha$) rather than risk yielding the initiative to its opponent. Only if A believes that B is plagued by high domestic opposition to the use of force is it likely that both A and B can escape violence and reach a negotiated settlement of their differences. Thus, theorem 1 reveals how dovishness can *heighten* the risk that force will be used, while theorem 2 shows conditions under which the expectation of dovishness favors peace. The two theorems together cast in a somewhat more optimistic light Midlarsky's hypothesis that "Political violence may occur when a unit (group, national region, or nation-state) has the capability to reduce uncertainty in its environment, but is constrained from doing so, and the uncertainty persists or increases. . . . The present framework specifies that only those with ability to reduce uncertainty yet are prevented from doing so will initiate violent acts with political intent" (1975, 37–39). Uncertainty, perhaps fortunately in our uncertain world, is sometimes a facilitator of peace.

Conclusion

The linkage between domestic opposition to conflictual foreign policy initiatives and the expectation that disputes will escalate to violence has been examined. Beliefs about domestic political opposition to conflicts proved not to be easily manipulated. Building a reputation as a nation with a relatively hawkish (or dovish) domestic opposition proves more difficult than one might have thought. Under the conditions stipulated here, we have found that nations cannot readily step out of character to deceive a rival into a course of action that otherwise would not have been taken. But we have also found that domestic politics is far from irrelevant in shaping international disputes.

Initial beliefs about the magnitude of a state's domestic opposition profoundly influence the course of events in crises. This certainly is a lesson demonstrated not only by our game but also by the U.S. experience in Vietnam and perhaps by the Soviet experience in Afghanistan. Our results seem to point to the conviction that honesty about domestic attitudes toward foreign policy is not only "ethical" but also strategic. It is apparently counterproductive for foreign policy leaders to pretend that their domestic political circumstances are different from what they really are.

Notes

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1. We also leave further elaborations of the game for future investigations. The game could, for instance, be extended from the nodes leading to war to include war maintenance, war termination, and so forth. Similarly, one could imagine bargaining extensions of the game from the negotiation nodes. For now, we assume that the game ends at the terminal nodes we have depicted.

2. In *War and Reason* (Bueno de Mesquita and Lalman n.d.) we evaluate the model also under the assumption that $\phi < 0$—in other words, that there are reputational, rally-round-the-flag or other domestic political benefits associated with using force. Here we focus only on this element as a net cost.

3. This technical assumption does not appreciably alter the implications of the results developed here. Although the threshold values of the equilibria shift without this assumption, the general characterization of the results remain the same. In *War and Reason* (Bueno de Mesquita and Lalman n.d.) we relax this assumption.

4. This need not imply that a single decision maker forms foreign policy. Rather, we have in mind that whatever the internal political process by which policy preferences become revealed, national policy follows from the ordering emerging from that internal process.

5. Our assumption stands in contrast to the assumption, common in many game-theoretic models of conflict, that the side with greater resources wins with certainty (see, for instance, Niou, Ordeshook, and Rose 1989). We prefer to assume a probabilistic view of war outcomes for several reasons. Of course, after the fact, one can always say that the winner must have had greater resources or power (Blainey 1973), but before the fact it is common for rivals to view their prospects in war as risky. Empirical assessments of war choices that are based on probabilistic models of war outcomes provide a stronger fit with the historical record than do models that assume outcomes are not risky (Bueno de Mesquita 1981; Bueno de Mesquita and Lalman 1986; Lalman 1988; Midlarsky 1988; Organski and Kugler 1980).

6. That is, the value of negotiations exceeds war under the lowest cost conditions.

7. We assume that fighting away from one's own territory is preferred to fighting at home, even though it is frequently asserted that defense is easier than offense in war. Differences in subjective probability estimates based on exactly this consideration, which are explored in detail in Bueno de Mesquita 1981 and Bueno de Mesquita and Lalman n.d., are not the focus of concern here. Although such considerations necessitate more complex specifications of payoffs than assumed here, they are not likely to alter the theoretical results we report. Rather, they are only likely to alter the likelihood with which certain preference orderings arise.

8. We wish to thank Philip Schrodt, who brought this quotation (*Time*, 16 July 1965, p. 25) to our attention.

9. In *War and Reason* (Bueno de Mesquita and Lalman n.d.), we fully explore all equilibria for all admissible orderings of outcomes by players *A* and *B*. It appears that two-way incomplete information is required for a war begun by nation *B* to be an equilibrium outcome of the game.

10. Although we stipulate these preferences throughout the remainder of our analysis, all of the results hold with the same proofs if *A* prefers that *B* capitulate rather than negotiate.

11. If the game repeated, *A*'s end-of-game knowledge might lead to different behavior on *A*'s part or on *B*'s part. However, in conflictual international interactions, a likely outcome of a single round of play is that the expected payoffs in future interactions will be altered by the results of the former interaction. Consequently, while the set of available strategies may repeat, the payoffs are unlikely to be repeated.

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THE POLITICAL MANIPULATION OF MACROECONOMIC POLICY

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Conventional wisdom and some research indicate that macroeconomic policies follow cycles corresponding to political, as well as economic, forces. Using vector autoregression analysis, I test three models of monetary policy determination for the United States, 1953–1984: the electoral cycle model (that reelection motivations on the part of presidents create a policy cycle), the party differences model (that policy changes reflect revolving presidential party administrations), and the referendum model (that changes in presidential approval create, in effect, a continuing referendum, allowing presidents to monitor their success and change macroeconomic policies when necessary). Analysis shows that monetary policies, as measured by the monetary base and short-term interest rates, respond to the election cycle and presidential approval (although the effect on macroeconomic outcomes is ambiguous). Party differences are found in real income but are not very significant in other variables.

Recent advances in positive political theory present a dilemma for democratic policy making. Aranson and Ordeshook (1985) outline a public choice view that attacks most efforts by the state to carry out programs capable of improving the general welfare of the public.[1] Policy analysts continue to produce masses of studies proposing various solutions to public sector problems, only to have potential programs changed, abandoned, or ignored by democratic political processes. Politicians, given constitutional and statutory rules, often find their self-interest at odds with the welfare of the public; and this eventuality is reinforced by the inconsistency of social choices (Arrow 1963) and the resulting inherent "disequilibrium" of politics (Riker 1982). The paradox is that policy analysts, such as welfare economists, search for answers to policy problems capable of making everyone better off; but democratic politics appears incapable of maximizing social welfare even when clear and simple solutions exist to public problems.

The political business cycle questions the ability of democracy to respond appropriately to macroeconomic policy problems. The political business cycle proposes that politicians find their self-interest best served by manipulating the economy in a way that maximizes votes. Longer-term policy concerns, such as price stability, are mortgaged in order to ensure that the public is satisfied with short-term macroeconomic performance when an election occurs. The advice of macroeconomic advisors becomes secondary to winning elections, and an apparent consequence is that everyone is worse off than if elections were not held.

Fortunately, these normative conclusions about the political business cycle rely on questionable and incomplete evidence. The influence of politics on macroeconomic policy choice has never been well understood. Ever since Kramer's (1971) seminal article linking macroeconomic performance to incumbent electoral fortunes, political scientists have speculated about how democratic electoral politics influences macroeconomic policy making. Tufte's *Political Control of the Economy* (1978), an award-winning classic on the political business cycle, inspired scholars to investigate the political business cycle further. Yet more than ten years after publication, little empirical evidence exists showing that either macroeconomic policy choice responds to elections or that unemployment and inflation follow a cycle corresponding with elections (Alt and Chrystal 1983). Just as the normative conclusions of the political business cycle present a paradox for democracy, the lack of empirical evidence produces an additional puzzle.

The state of knowledge is reflected by Alt and Chrystal, who denounce the political business cycle as void of empirical support: "No one could read the political business cycle literature without being struck by the lack of supporting evidence. There must be cases where politicians have undertaken electorally motivated interventions. . . . But while this happens . . . such cases may be trivial in comparison with other economic fluctuations" (1983, 125). Woolley concludes similarly about monetary policy: "In terms of elections, it is very clear that the Federal Reserve is not indifferent to what is going on. There is almost no persuasive evidence that the Federal Reserve is actively engaged in partisan manipulation, at least not of the sort that can readily be detected," (1984, 129). These conclusions are not without some detractors (e.g., Grier 1987, 1989), but evidence weighs heavily against the political business cycle.

Given these findings, political economists have focused on alternative models for explaining the politics of macroeconomic policy. The most popular is Hibbs's (1977) model of partisan differences in macroeconomic policy, a model that has fared more successfully under empirical scrutiny. However, this model is not inconsistent with the political business cycle model; thus, support for the party differences model does not imply that the political business cycle model is necessarily wrong. Thus, the search for evidence continues.

The enduring interest in political explanations of macroeconomic policy has been fueled by the increasing concern of economists. Not only are political scientists concerned with the influence of politics on macroeconomic policy, but many macroeconomists have been forced to consider the power of politics. Sims speaks of a political pendulum that voters and economic agents understand and react to (Sims 1982, 118). Lucas's (1977) concept of a policy regime requires a sophisticated view of the relationship between the government and the economic-political agent. And Paul Volker, exiting his position as Chairman of the Fed, was forced to answer questions before Congress about the likelihood that his successor, Alan Greenspan, would increase the money supply in the coming election year. Thus, macroeconomic policy analysts are interested in the effects of politics on policy and acknowledge the importance of determining appropriate political models of the macroeconomy.

The statistical model offered below is an effort to fill the gap between theory and empirical knowledge on how elections help explain macroeconomic policy choice. I analyze a small-scale vector autoregression model of the U.S. macroeconomy. My main intention is to isolate and identify possible sources of presidential influence on monetary policy and to evaluate the effects of political influences on macroeconomic performance. Evidence from this analysis suggests that presidents influence monetary policy in response to electoral and short-term political needs. Less support is provided for a model in which policies differ as a result of the ideological orientations of Democratic and Republican presidents.

The most striking finding is that elections are benevolent. The electoral cycle in macroeconomic variables at best reflects general improvement in all economic indicators and at worst suggests that an electoral cycle in interest rates has no adverse effects on macroeconomic outcomes. Although the data implies that elections are not a cause for concern about the economy, they do provide robust evidence that the continuing referenda presidents must face, given continuous polling of the public, result in monetary policy responses sensitive to the president's public support.

Three Models of Macroeconomic Policy Determination

I shall focus on monetary policy as a major indicator of the direction and intensity of macroeconomic policy. Money is regulated by the quasi-independent Federal Reserve, yet the president must try to coordinate macroeconomic policy. Also, it is the president whose political fate rises and falls with successes and failures of monetary policy. High interest rates and inflation hurt Carter's reelection efforts in 1980, and decreasing inflation and expectations of falling interest rates helped Reagan in 1984. Thus, given that some policy choices are capable of sending an incumbent to defeat when other policies might produce victory, an important question concerns whether presidents actively try to influence monetary policy for political benefit.[2]

Conventional political-economic hypotheses require that the president influence monetary policy in some way. The Federal Reserve's independence is reason enough to question the president's importance in determining monetary policy. However, there is very little evidence of direct political conflict between the president and the Federal Reserve, and the relationship is more cooperative than conflictual (Woolley 1984, 111–114). If the Federal Reserve understands the political needs of the presidency, it may be willing to help the president. As Wooley states, "If indifference is a hallmark of independence, then the Federal Reserve is not independent" (1984, 129).

Three models of monetary policy are evaluated below: the *party differences*, *electoral cycle*, and *referendum* models. The first two have been widely tested by a variety of scholars; the third has been all but neglected as an explanation of macroeconomic policy choice.

The party differences model, first defended and tested by Hibbs (1977), proposes that coalitions favoring one party over another have distinct macroeconomic policy objectives that they expect their preferred party to share. Parties should reflect their constituencies' preferences through policy choices. The key empirical question concerns the role of parties in offering macroeconomic policy platforms and actions distinct from one another. The party differences thesis is quite simple. Parties attempt to build coalitions of voters; in their doing so, macroeconomic policies will correspond to the preferences of these coalitions. Hibbs (1977, 1987a, 1987b) documents the expected differences of leftist and rightist parties. In the United States, Democrats should respond to populist concerns, namely, that unemployment be lowered even at the expense of higher inflation. Republicans should respond to higher-income constituents and business groups, which have traditionally been more inflation-averse than others. Therefore, policies oriented toward stabilizing prices should predominate under Republicans. While the informal, historical evidence is somewhat confusing, Hibbs's formal, statistical evidence indicates that Democrats have held the rate of unemployment significantly lower than Republicans.[3]

The political business, or electoral cycle model, of which the political monetary cycle is a consequence, was first proposed by Nordhaus (1975) and extensively investigated by Tufte (1978).[4] By stimulating the economy prior to elections, incumbents can tip the balance in their favor because voters favor expanding or growing economies. Likewise, recessions give challengers potent ammunition that is difficult for an incumbent to repel. The electoral cycle model predicts that monetary policy will be more expansionary as incumbent presidents concern themselves with reelection. Exactly how presidents would do this and what the timing might be is open to question. Little evidence about direct contact between the Fed and the president concerning electoral manipulation exists, though Tufte's work does an excellent job of documenting the existence of a political monetary cycle in 1972. Because informal evidence is scant, one must turn to formal statistical evidence of a political monetary cycle. The most sophisticated work of this type comes from Beck (1987) and Grier (1987, 1989). Beck shows that monetary policy is not directly influenced by elections (though the Fed might respond to fiscal policy changes in a way that produces a cycle indirectly). However, because spending increases are not strongly related to macroeconomic variables such as inflation and unemployment, focus on monetary policy is essential if a president is to create a political business cycle. Furthermore, any effects of federal spending on macroeconomic outcomes may take much time before influencing macroeconomic variables. In addition, control of federal spending by the president is limited, spending initiatives take time to implement, and the history of large budget deficits places constraints on large-scale spending initiatives. Grier was more successful than Beck in finding a political monetary cycle within the context of a monetarist model. Meiselman (1986) also finds evidence for an electoral cycle in monetary policy, but he concedes that the model may not be appropriate for all presidential administrations.

The referendum model hypothesizes that monetary policy will reflect short-term changes in the president's approval rating, with opinion polls acting as a referendum on presidential policy. Frey and Schneider (1978a, 1978b, 1979, 1981) investigate models of macroeconomic policy hypothesizing that the government's performance rating influences policy choice. In trying to manipulate the public's satisfaction with their regime, government policy makers will respond to changes in the government's performance rating by changing policy. Likewise, presidents go through periods where they must try to improve their public support; and the continuing referenda measuring their performance rating with the public allows (requires) presidents to monitor public opinion. Presidents can try to manipulate the public through the media, but policy changes that are more consistent with those of the governed may be necessary to improve the president's support. Therefore, presidents may be more likely to follow "populist" policies when in political trouble; that is, macroeconomic policies should become more expansionary as a president's public approval rating declines. Short-run manipulation will occur because presidents must increasingly manage their public support, as this support is linked to presidential success and prestige as well as to future electoral prospects (Ostrom and Simon 1985).[5] Political scientists focus squarely on the determinants of presidential approval, yet little formal or statistical research considers the role of feedback from performance ratings to policy.[6] And no one has estimated the effect of this public opinion feedback on monetary policy choice.

The policy differences model proposes that parties make distinct macroeconomic policies; the electoral model accounts for monetary policy choices as a function of the electoral cycle; and the referendum model predicts that presidents will influence monetary policy in response to changes in approval scores. These three political explanations of monetary policy are all testable within the context of some macroeconometric model.

Methodological Considerations

Any statistical model of the macropolitical economy must account for aggregate relationships among macroeconomic variables as well as relationships among political and economic variables. Politics should influence policy choice; but because the United States is a capitalist democracy in which demand and supply for goods and services are largely determined in private markets, political variables do not perfectly determine the paths of economic variables. Hence, specification of politics in a statistical model must not ignore the private nature of the political economy, as macroeconomic policy is meant to alter private economic behavior, not replace it. However, political choices can directly affect private markets, as expectations of policy actions influence economic agents. Thus, a very complex, interdependent view of the political economy unfolds; and this complexity requires a shrewd statistical model-building strategy.

Conventional econometric practice calls for use of theory to specify and estimate a parsimonious, structural macroeconomic model that includes political factors.[7] Unfortunately, econometric convention has been called into question for just this type of modeling exercise (Sargent 1981; Sims 1980, 1982). Structural equation methods fail adequately to model the dynamics of the macroeconomy in at least three ways (Sims 1980): (1) many zero-order restrictions used by econometricians are ad hoc and not based on sound economic theory; (2) identification of dynamic structures usually requires that the exact order of serial correlation in the errors and the exact lag distributions be known; and (3) rational expectations models are much more difficult to specify than conventional macroeconomic models and require even more elaborate and dubious zero-order restrictions for identification.

The place of zero-order restrictions in dynamic model building is best understood as an econometric convention developed for use in cross-sectional model building. In the 1960s most of macroeconomics was given a static representation, and econometrics was primarily oriented toward analysis of cross-sectional data.[8] Restricting coefficients to equal zero is required to identify a structure from the cross-sectional covariance matrix, and these types of restrictions also prove useful for estimating parsimonious regressions. Theory generates a substantial number of zero restrictions needed to estimate most static structural equation models: without these restrictions, most static systems are impossible to estimate. However, this widespread use of zero-order restrictions does not translate well to dynamic models, and large-scale dynamic macroeconometric models suffer accordingly. Present knowledge of time delays, expectations, and market sector interactions simply is not capable of generating the types of restrictions needed to identify a conventional dynamic econometric model. As Sims (1980) argues, the types of restrictions used to generate large-scale models of the macroeconomy are ad hoc because economic theory does little to suggest many of the restrictions. The dynamics of economic variables are rich, but most zero restrictions made as a result of conventional econometric practice ignore this richness.

Hatanaka (1975) shows that identification procedures for structural equation models usually rely on the assumption that the order of serial correlation in the error terms and exact lag distributions are known. If either of these dynamics is unknown, as they usually are, the order condition for identification becomes more stringent. Intuitively, the dynamics of the structural model must be accurately accounted for so that the assumption of predeterminedness of some regressors holds. For all practical purposes, the Hatanaka result makes impractical attempts to model the dynamics of the macroeconomy using conventional methods, as these approaches ignore too much uncertainty about dynamic interactions among variables.

Rational expectations wreaks havoc on specification and identification procedures used in structural equation modeling.[9] There exist many rational expectations models, none of which have gained widespread recognition as the single model of rational expectations for a given subfield of macroeconomics. Sargent (1987, 2) calls these "general equilibrium" models "special," reflecting the idea that there is an entire class of rational expectations models. Rather than signifying a single model, rational expectations suggests a certain logical consistency among variables in a model. Economic agents seek information, and the relationships among variables described by a statistical model should allow these agents to predict important variables. An inconsistent model ignores this connection between rational expectations formed by economic agents and predictions implied by a model. Because many different rational expectations models may allow for this consistency, the dynamic implications of rational expectations are inherently uncertain. Sims (1980) actually argues that the logic of rational expectations makes model identification infeasible, since the behavior of economic agents and policy makers will conspire to make structural model building impossible.

Rational expectations also makes risky any straightforward interpretation of structural parameters. If variable A is a good predictor of variable B and economic agents are trying to maximize a utility function that varies according to variable B, A becomes important in enabling economic agents to make rational choices. Sims (1982) shows how rational expectations can generate a money-to-income causal ordering if individuals have dynamic money demand functions. Sims's model shows that macroeconomic variables other than asset prices can behave like asset prices (Fama 1970), which leads to cautionary interpretations of causal orderings among macroeconomic variables. The possibility of rational expectations requires cautious interpretation of dynamic models.

All of these criticisms of conventional econometric practice suggest more subtle methods for modeling dynamics. In this application, a variety of factors affect dynamics among variables and must be accounted for. First, aggregate supply-and-demand schedules of private economic agents are important, as are agent expectations of future supply, demand, and policy behavior. Second, policy actions that are a function of political motivations imply additional dynamic relationships among political and economic variables. Third, policy movement that is a result of macroeconomic policy analysis rather than political considerations can also influence dynamics.[10] Objective, "nonpolitical" policy response cannot be ignored, as politicians have preferences for particular mixes of policy.[11]

Analysts such as Chappell and Keech (1988a, 1988b), Alesina and Sachs (1988), Alesina (1988), and Beck (1987) attempt to deal with uncertainty about structural equation models through analysis of alternative specifications. Vector autoregression (VAR) is an attempt to deal with these same problems through analysis of an unrestricted reduced form of some unknown, underlying structure. The strength of VAR is that it describes fully the dynamics of the macroeconomy, whereas conventional structural approaches leave much of the dynamics in the disturbance terms of the equations. Even if a political-economic hypothesis is tested within the context of more than one structural model, findings are still contingent on one of the structures' being a realistic representation of the macroeconomy. Vector autoregression is particularly useful for evaluation of political-economic hypotheses because political economists are essentially interested in how political variables influence the dynamics of the macroeconomy. Thus, VAR describes these dynamics; and if the political-economic hypotheses are valid, political variables should explain dynamics economic variables cannot.

Rational expectations macroeconomic models are even more difficult to specify than conventional macroeconomic models, and VAR analysis is very appropriate for modeling relationships among economic variables subject to rational expectations (Sims 1980). Minford and Peel (1982), Rogoff and Sibert (1988), and Alesina (1988) have investigated political-economic rational expectations models, but VAR allows evaluation of political-economic models without proposing one of very many possible specifications of the dynamics among economic variables in a rational expectations model. In summary, analysts need not know the exact economic model (whether it is a rational expectations model or not) to evaluate political-economic hypotheses with VAR. This makes VAR a particularly useful method in light of the persistent disagreement among economists about macroeconomics (Frey et al. 1984).

The Vector Autoregression Model

Empirical model building efforts must address two related desiderata. First, any methodology we use should be able to account for the historical variation in the data resulting from regularized behavior of policy makers and political-economic agents. The exact model that can account for this data is unknown, but an appropriate methodology should account for all sources of dynamics among the variables being analyzed. In fact, different theoretical models usually imply different dynamics among variables, and the statistical model should not privilege one theoretical model over another. Second, the methodology should allow specification and interpretation in terms of the political-economic hypothesis. The statistical model should enable understanding of the interrelationships among variables in the political economy in a way that refers directly to the political models. Vector autoregression analysis is used because it conforms to each of these criteria. (For detailed treatments of the methodology, see Freeman, Williams, and Lin 1989; Litterman 1984; Shamir 1983; and Sims 1980.[12])

A VAR model consists of a relatively small number of variables, each represented by an equation. Each equation has the same variables, all variables are in each right-hand side, and enough lags are in each equation to fully represent the time series; that is, the lags included in the equations should account for enough variance that we can assume the regressions approximate infinite lagged VARs. The lag lengths themselves are chosen empirically, to make sure that the process approximates an infinitely lagged dynamic system. This ensures a valid description of the dynamic properties of time series data.

The vector autoregression can be written,

$$Y(t) = CX(t) = \sum_{i=0}^{I} B(t - i)Y(t - i + 1) + e(t),$$

where Y(t) is an $M \times 1$ vector of time series, C is a $M \times L$ matrix of coefficients, X(t) is an $L \times 1$ vector of deterministic variables, B(t) is an $M \times M$ coefficient matrix, the elements of Y$(t - i + 1)$ are the values of Y(t) lagged $i + 1$ times, and e(t) is an $M \times 1$ vector of disturbance terms. The major restriction on fitting a VAR is the number of variables that can be included in the system, because the number of estimated parameters increases quadratically as the number of variables increases linearly (the total number of estimated parameters is $M \times M \times l$). Omitted variables can change the dynamics of the system. Thus, choice of variables is important (though experimentation with variable selection can generally ease fears of omitted variable bias).

Once the VAR is estimated using ordinary least squares,[13] interpretation of the system's dynamic properties is necessary. Two devices I use are *tests of causal priority,* often referred to as *Granger causality tests,* and *innovation accounting,* a procedure that inverts the autoregression into a moving average process capable of describing the system's response to shocks (innovations) in variables.

Causality tests are useful for testing specific hypotheses about dynamic cross-equation independence (Freeman 1983; Sargent 1981; Sims 1977); that is, some hypotheses may suggest that one variable is causally prior to another, and these tests allow for description and inference about causal priority in the data.[14]

The moving average representation (MAR) of the VAR is used to describe the dynamics of the data. The vector of time series can be written,

$$\mathbf{Y}(t) = \sum_{i=0}^{\infty} \mathbf{A}(t - i)\mathbf{e}(t - i),$$

where each A$(t - i)$ is an $M \times M$ matrix of moving average coefficients. A series can be explained as a sum of all past shocks or errors. Innovation accounting using the MAR of a VAR process is similar to dynamic multiplier analyses used in structural equation modeling, although all variables themselves are endogenous. Also note that the moving average representation ignores deterministic variables.

The system's dynamics resulting from an innovation or random shock in a variable are described by the MAR. A random shock is exogenous, so it makes sense to trace out the response of all variables in the system to a random shock in any variable. The response of a variable to a shock shows a pattern of possible influence across variables. A particular theory may require a certain dynamic pattern; thus, the theory can be appropriately evaluated. The importance of a response can also be evaluated by decomposing the hypothetical forecast error of a variable into parts due to forecast errors in other variables. If one variable causes another, its forecast errors should cause forecast errors in other variables.

The following statistical analysis uses VAR to analyze movements of policy and economic variables in response to changes in the party of the president, elections, and changes in the president's approval rating. Vector autoregression is particularly useful for estimating the influence of approval on the macroeconomy because macroeconomic outcomes also influence the public's evaluation of presidential performance. Vector autoregression controls for past macroeconomic performance; therefore, any impact of approval cannot simply be dismissed as an artifact of the association of high approval with good economic conditions.

A seven-variable model of the U.S. macropolitical economy was estimated for quarterly data series running from 1953 through 1984. The endogenous variables making up the Y vector are the monetary base, the three-month treasury bill auction rate, real gross national product, unemployment, GNP price deflator, federal government expenditures, and presidential approval. This choice of policy variables focuses on the monetary sector. Quarterly data are used because real GNP and federal expenditures are not measured for shorter periods.[15]

More elaboration is necessary regarding the choice of fiscal policy variable. I use aggregated federal expenditures because this variable is meant to account for fiscal pressures on monetary policy, not to trace out spending changes resulting from budgetary and discretionary political choices—analysis which would require a more disaggregated measure. I choose spending, rather than revenues or the deficit, on grounds that spending decisions (with the possible exception of the last few years) have exhibited more discretion, thereby sending more information to the Fed as an indicator of fiscal pressures on monetary policy.

The seven-variable VAR model is estimated with the addition of five deterministic variables, making up the **X** vector. A dummy for the first quarter of each presidential term is used to account for the high approval rating new presidents enjoy at the beginning of their tenure. A variable for the presidential election term is used, linearly growing from 1 to 16 over a four-year term. Another variable, from one to eight, increases linearly over congressional election periods. A party dummy is used, coded zero for Republican presidents and one for Democrats. World petroleum prices are also included to account for movement in macroeconomic variables resulting from major supply shocks, thus explaining dynamics among variables that might otherwise appear anomalous.[16]

The use of two linear trend or counter variables to specify electoral effects is meant to overcome problems associated with using discontinuous dummy variable specifications as found in Hibbs (1987a), Beck (1987), and Alesina (1988). First, these types of election specifications have been criticized by Grier (1987, 1989), who points out that the specifications require a drastic and discontinuous falloff after elections.[17] Second, conventional dummy variable specifications involve an incredible amount of uncertainty as to the startup period and the overall shape of the response. In addition, the use of a congressional election variable is not common, but it is useful for uncovering more complex dynamics resulting from the natural interaction of presidential and congressional elections (Alesina and Rosenthal 1989).

The use of two trend variables helps overcome these problems by specifying a continuous response function in which the importance of elections is assumed to be linearly related to the distance from the next election. Discontinuities resulting from term changes are much less important within the VAR framework, since the VAR accounts for all possible dynamics among economic variables and the election terms are meant to account for variance not explained by dynamics among economic variables.[18] In addition, this specification has the strength of being consistent with a more elaborate optimal control framework in which preferences of policy makers for various economic outputs are a function of the distance in time from the next election (see Friedman 1975).

Alternative distributed lag specifications using the party dummies and election trends lagged through four quarters were not found to significantly increase fit or change substantive findings. In addition, using more complicated polynomials in place of the linear election trend terms also failed to increase fit or change findings significantly. Thus, the results I report using quite simple specifications are robust to more complicated alternatives.

Interpretation of the VAR analysis proceeds as follows. Statistical tests about the

Table 1. Coefficients and t-Ratios for Party Difference Dummy Variables

Equation	Coefficient	t-Ratio	Maximum Dynamic Effect
Approval	-.189	-.19	3.10
Monetary base	-.00013	-.07	.02
Interest rate	-.171	-.77	-.19
Unemployment	.0229	.24	.77
Prices	.0013	.90	.02
Federal expenditures	-.000017	-.00	.04
Real GNP	.00376	1.71	.03

Note: All entries are from the seven-variable VAR described in the text. The coefficients represent the difference between Democrats and Republicans, with positive values indicating that Democratic administrations are larger. The maximum dynamic effect represents the maximum difference in each variable attributable to party differences. These values come from the differences in two simulations, one run for Democratic administrations, the other for Republicans.

influence of presidential party and elections on each economic variable are presented. A simulation is performed to evaluate the importance of the presidential party dummies and the election trend terms on the monetary base and interest rates. To assess the importance of presidential approval in shaping macroeconomic policy, I present F-tests from the VAR to detect if approval helps in predicting any of the variables in the VAR.

Moving average representation analyses trace out the implied dynamics of the system in response to shocks in approval, with the expectation that negative approval shocks will lead to more expansionary monetary policy. The response of approval to shocks in economic variables allows evaluation of the "approval function" describing the relationship between macroeconomic performance and presidential approval ratings.

Results

Party Differences Model

What evidence is there that the differences in policy doctrine espoused by Republican and Democratic presidents result in distinctive policies? Estimates of the party coefficient for each equation in the VAR describe the difference between a Democratic and Republican administration for each variable. Significance tests are useful for assessing the significance of the direct effect of party in each equation. The magnitudes of the effects are best obtained by simulating the dynamics of all variables for two scenarios: one in which the party variable is held constant at one (for Democrats), and another in which the party variable is held constant at zero. Differences in the simulated variables reflect the differences that are due to party alone, and the maximum effect gives the strongest difference within a four-year period. This method is preferred to interpretation of the direct effect (as represented by each coefficient) because feedback within the system will in general be an important source of variation on all variables.

Table 1 presents t-values for null hypotheses that Republicans and Democrats are no different in either the policies they pursue or the outcomes they generate. The results provide limited support for Hibbs's (1977, 1987a, 1987b) analysis. Hibbs finds quite significant differences across administrations for unemployment and real output, but my analysis shows that the only effect close to being statistically significant is on real income. In fact, the direct effect on unemployment is both extremely small and quite insignificant.

The real income effect is of the same magnitude as Hibbs found: three percentage points difference in growth between Democratic and Republican administrations. However, the unemployment effect, as simulated using the VAR, is positive and small, indicating that Democrats hold unemployment higher than Republicans. In all, this analysis hardly supports conventional wisdom.

It is well understood that VAR analysis compromises increased potential precision of estimates in exchange for a full description of dynamics. These statistical tests are not very powerful. Thus, it is useful to look for other large dynamic effects of party administrations even if the coefficients are insignificant. Of the policy variables, money growth is 2% larger under Democrats, and prices are also 2% higher under Democrats. Though the effects are small and we should not make too much of these, both differences are in a direction that suggests the Democrats might follow a more expansionary monetary policy. However, the primary anomaly is the positive unemployment difference, a finding that is inconsistent with the findings of others. The additional findings are not altogether inconsistent with those of other scholars who use a variety of econometric techniques and macroeconomic models, although the magnitudes are sometimes smaller for my analysis.[19]

Three major conclusions about the party differences model can be made. First, I find a sizable difference between parties in real income and smaller differences in money and prices (within the sample), all in the expected direction as hypothesized by Hibbs and others. Democrats are more concerned with short-term expansion and growth than Republicans, and Republicans are more inflation-averse. However, given the weak statistical significance of all but one of these results, we must guard against out-of-sample generalization and treat the results as historical differences. The second major conclusion is that the VAR results show no meaningful differences between unemployment rates, a finding that contradicts not only Hibbs but also Chappell and Keech (1988a; 1988b) and Beck (1982b). Third, as measured by the monetary base, there are some differences in monetary policy across parties even though interest rates are only slightly lower under Democrats. The small differences are consistent with Beck (1982a), who argues that larger, more significant differences can be found across administrations, with party effects much less important.

These results are comparable to those of other scholars with the exception of the unemployment finding. The reason for the differences in unemployment results in comparison to others (especially Hibbs) can be attributed to method. VAR accounts for more dynamics among macroeconomic variables than do other structural and nonstructural statistical representations; thus, spurious findings of differences across parties are less likely using VAR. Any dummy variable can account for residual variance created by omitted variables, and with VAR, less variance remains in the residual. Thus, much of the differences in unemployment across parties found by other analysts must be due to dynamics among economic variables that those analysts failed to model.

The findings on party differences are sometimes very large, though in all cases they are statistically insignificant. The insignificance of the differences raises doubts about the ability to make generalizations from the party differences model to other (future) samples and suggests that these differences are sample-specific, an interpretation that supports Beck's (1982a, 1982b) basic contention that differences across administrations are more

Table 2. F-Ratios for Election Trend Variables

Equation	F-Ratio	P-Value
Approval	2.14	.13
Monetary base	.66	.52
Interest rate	3.82	.02
Unemployment	.55	.58
Prices	.49	.61
Federal expenditures	.20	.82
Real GNP	.29	.75

Note: Each entry is the F-ratio for a test of the null hypothesis that both of the election coefficients are zero. The distribution under the null is the F-distribution with (2,74) degrees of freedom.

relevant than differences across parties. Nevertheless, parties do exhibit some differences within the sample, indicating that they are capable of generating alternative policies to the policies of those that preceded them.

Electoral Cycle Model

The reelection motivation will be shared by both parties, and the historical variation in macroeconomic policy should reflect this agreement if some coherent viewpoint regarding vote-garnering strategies exists. Tufte (1978) and Nordhaus (1975) both present persuasive arguments that the public is averse to high levels of unemployment and low levels of economic growth. Both authors agree that this will lead a president to favor expansionary macroeconomic policies before elections, as voters will be informed enough to see economic improvement but too myopic to see the long-term inflationary consequences of too much expansion. These arguments are based on a Phillips Curve relationship between unemployment and inflation, a trade-off that most macroeconomists now believe is not universally available to policy makers.

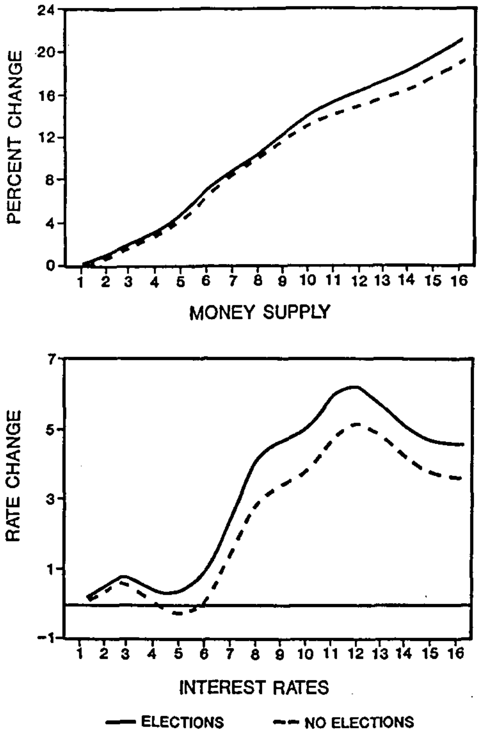
Even without relying on the Phillips Curve for support, however, the idea that expansion means more jobs, better expectations about economic growth, and a general optimism about the economy is undeniable. The reverse side—contraction brought about by tight monetary policies—leaves individuals with a pessimistic view of economic prospects. No president will be willing to fight inflationary tendencies in an economy quite as hard near election time as he would early in his term. A presidential challenger would much rather have a recession as ammunition than the expectation of higher prices. Thus, on average, we would expect monetary policy as measured by money and interest rates to be more expansionary, the closer an election.

Tests of the hypothesis that at least one of the coefficients for the election trend terms is nonzero yields the statistically significant finding that interest rates are in part explained by the election cycle (Table 2). In only one other equation—the approval equation—does this hypothesis test approach statistical significance. Thus, the results of the VAR analysis indicate that if an election cycle is generated, changes in short-term interest rates drive the cycle. That the direct effect of elections is reflected in interest rates, not the monetary base, is consistent with the operating procedures of the Federal Reserve—at least through 1979, when pegging interest rates as the intermediate target was replaced with monetary targets.

As in the case of interpreting the party differences model, merely interpreting the coefficients of the election trend terms or assessing their statistical significance fails to reflect the natural dynamics of economic variables and presidential approval over an election cycle. The VAR is a reduced-form system of equations; and many other variables may also cycle, in part due to the cyclical nature of interest rates. We can exploit the fact that election trends are deterministic variables and perform a simulation of the dynamics implied by the VAR for the election cycle. Simulated values for all seven variables can be generated, allowing the election trend variables to take on their natural values over a four-year election cycle. Additional simulated values can also be produced by holding the election trend terms constant at their initial value, thereby allowing comparison between a system where elections affect dynamics and one where elections cannot influence dynamic interactions among variables. While this does not tell us what would happen if elections were not held, it is the best method for describing the effects of the election variables on the dynamics of the system.[20]

Figure 1 presents simulated values of the monetary base and interest rates over four years under two different scenarios. The dashed lines show what we would expect if the election trend terms remain at their initial, first quarter value. These simulated values describe the dynamics of the data holding the election terms constant. The solid lines show the values these variables take over a term allowing for the influence of the election variables.[21] Monetary policy is more expansionary over a four-year presidential term as a result of elections, whether one favors measuring policy with interest rates or the monetary base. The difference for interest rates becomes nearly 1%—a nontrivial difference—and the difference in growth rates of money nears 3% after sixteen quarters.[22] If one believes that money and interest rates are related as a result of historical policy behavior of the Federal Reserve, these results are difficult to explain as a function of anything but electoral motivations on the part of the president and other economic policy makers. Why else would this dynamic pattern emerge from the data?

Figure 1. Simulated Values of Money and Interest Rates over Two Hypothetical 16-Quarter Periods



Note: These simulations were produced using the seven-variable VAR described in the text. The dashed lines represent simulated values of the endogenous variables holding both election variables constant. The solid lines represent simulated values of the endogenous variables while allowing both election variables to vary freely. The differences in each response for each graph are due entirely to the differences in the values of the election variables.

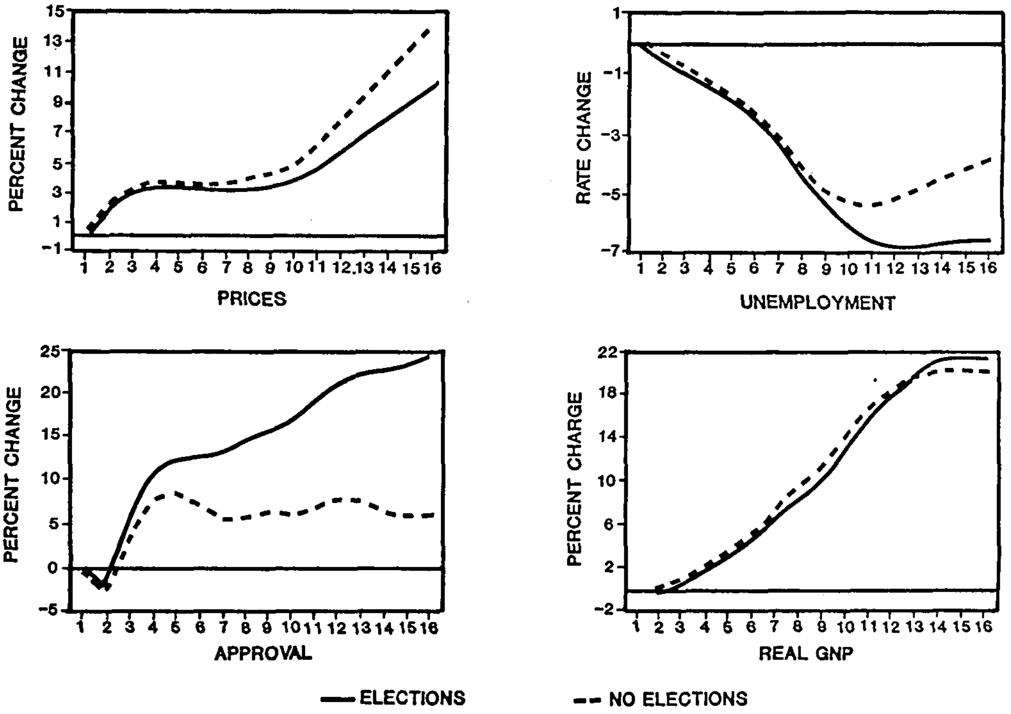
The existence of electoral cycles in monetary policy variables answers the question of the existence of a political monetary cycle but fails to address the question of how the cycle is created. Is there explicit collusion between the president and his economic advisors and the Federal Reserve Board? Does the Fed merely reflect the political motivations of the elected president, thereby reducing its own independence? Or is the making of macroeconomic policy so interdependent that the Fed must give in to action of the executive as a matter of necessity, not choice? These questions are not answered by merely showing the existence of a political monetary cycle.

One explanation of the existence of a political monetary cycle suggests itself. Perhaps the statistical result is a function of the Fed's proclivity to "lay low" during elections (Maisel 1973, 117; Woolley 1984, 127–29). Woolley shows that during election years highly visible policy moves are much more likely to be expansionary, with many fewer visible contractionary moves. This explanation is consistent with a view that the Fed itself causes a political monetary cycle even though it does not particularly care if a president is reelected or a new president is of the former president's party. It simply does not want to be criticized in a period of high visibility. However, this behavior is nevertheless the result of the president's reelection motivation, because if the president did not care about the direction of monetary policy, the Federal Reserve would not try to "lay low" during elections: without elections this cycle would not exist. Thus, one model of implicit collusion can account for the existence of the political monetary cycle.

An electoral cycle in monetary policy indicators is not enough to substantiate the existence of a political business cycle of the type Nordhaus and Tufte describe. Policy actions may have little effect on macroeconomic indicators. Figure 2 presents the responses of prices, unemployment, and real income over the two simulated four-year periods. Also displayed is the movement of presidential approval. Remarkably, when the election trends are allowed to vary, each economic indicator shows improvement over the term as compared to the simulation holding the election trends constant. After 16 quarters—theoretically equivalent to the quarter of a presidential election—real income is 2% greater than when the election trends are held constant (about one-half standard error of the simulation), and unemployment is more than 2% lower (almost two standard errors). The dynamics of real income are consistent with those predicted by the political business cycle in that income is higher at the end of the election term and lower at the beginning than in the corresponding periods of the nonelection simulation. Unemployment and prices (about 3.5% lower and two standard errors different for the election simulation) are lower during all periods of the election simulation, a result that has uncertain implications for the political business cycle argument.[23] Both Tufte and Nordhaus argue that price increases will result from trying to keep unemployment too low, but these arguments rely on a Phillips Curve interpretation. There is no evidence that inflation is higher during the first few quarters after an election cycle, a prediction that both Nordhaus and Tufte make based on a Phillips Curve relationship between unemployment and inflation.

The major implication of this analysis is that there are no obvious costs to the electoral cycle in interest rates. In addition, the response of approval over the election term simulation is very strong, with a difference of 18% at the end of the simulation period.[24] This remarkable difference is relatively easy to understand within the context of this VAR analysis, although it does contradict models that indicate approval declines (Mueller 1970) or declines and rises (Stimson 1976) over an election period. Presidents spend a vast amount of time and resources managing public support because of elections. This should be reflected in the simulation, because in it the dynamics of the economy and approval are controlled for, even though feedback does influence the movement of all variables. However, much of the movement in approval is not due to changes in economic conditions, and much of this movement must be seen as the influence

Figure 2. Simulated Values of Prices, Approval, Unemployment, and Real Output over Two 16-Quarter Periods



Note: These simulations were produced using the seven-variable VAR described in the text. The dashed lines represent simulated values of the endogenous variables holding both election variables constant. The solid lines represent simulated values of the endogenous variables while allowing both election variables to vary freely. The differences in each response for each graph is due entirely to the differences in the values of the election variables.

of elections on presidential efforts to manage approval. However, some of this difference in approval across the simulated periods can also be attributed to better economic conditions.

As a whole, these VAR findings about the U.S. election cycle contrast very strongly with conventional wisdom and somewhat less strongly with current and past research on the political business cycle. Tufte (1978) and Nordhaus (1975) did not subject their models to rigorous tests within the context of an econometric model, yet their description of the political business cycle remains prevalent. However, within the context of the St. Louis monetarist model, Grier finds "an almost perfect example of a Nordhausian [political business cycle]" (Grier 1989, 385), with cycles in prices and real income resulting from a political monetary cycle.[25] But the specification of Grier's "reduced-form" model is very restrictive, limiting the amount of dynamics explained by the right-hand-side variables of his money equation. He admits that different specifications may give different results; and his final conclusion merely argues that his results make the political business cycle a plausible model in contrast to Beck's (1987) findings that monetization of debt is the true dynamic behind a political monetary cycle.[26] With the exception of Grier, little research supports the electoral model in both policy instruments and economic outcomes (Alt and Chrystal 1983, chap. 5).

The VAR analysis I have reported represents a major contrast to most past research on the electoral cycle, not only in terms of the findings but also in terms of the methodological approach. The findings differ from those reported elsewhere primarily because the method emphasizes the accurate description of dynamics among economic variables, whereas past research has focused on relatively parsimonious structures or reduced forms. Furthermore, the election specification in this VAR accounts for policy actions throughout the entire election cycle rather than focusing more specifically on policies made during an election year, as other specifications do. With this more general specification, election cycles in the data are uncovered even if they do not correspond to theoretical preconceptions. Allowing for unexpected dynamics is particularly important in modeling cycles in macroeconomic outcomes because conventional predictions about political business cycles rely on dated Phillips Curve models of macroeconomic policy. Vector autoregression models will adequately describe dynamics even if the Phillips Curve is expectational.

One notable disadvantage of this VAR analysis is that the estimated cycles are relatively imprecise—more parsimonious models generally produce estimates with much greater precision. But this precision is illusory unless the estimated model is relatively well specified. The VAR presents a more accurate view of uncertainty about the political macroeconomy than do other statistical models, and it also gives a description of the dynamics of economic variables within the sample.[27] In addition, the description is not specification-specific. Thus, from a social-scientific viewpoint, the apparent disadvantage of imprecision becomes an advantage. In short, the VAR model gives a more modest assessment of the probability structure of the political economy—an assessment that is more realistic because it does not depend on ad hoc specification decisions.

Referendum Model

The electoral simulations depict the dynamics of the macroeconomy after all other dynamics are accounted for. One dynamic not directly described by the simulation is the influence of presidential management of approval ratings on short-term changes in monetary policy and economic outcomes. Do the fortunes of the president as reflected in his approval ratings create different policy responses, given similar economic circumstances? One way to answer this question is to trace out the response in macroeconomic policy variables to an innovation in approval using the MAR of the VAR. This simulation differs from those performed to evaluate the party differences and electoral cycle models because approval is endogenous and innovations must be introduced into the system. If no change in policy occurs as approval changes, the response plots will be flat. If, however, an innovation in approval—set as a one-standard-deviation unit of the disturbance terms of the approval equation—produces policy action as a result of the president's trying to manage his public support, the response of policy variables to these innovations will describe this process. A negative approval shock will result in efforts to expand the economy or fewer efforts to fight inflation with contractionary policy. Since the VAR model is symmetric, a positive approval shock should result in the opposite response, the inter-

Table 3. F-Ratios for Block Triangular Restrictions Involving Presidential Approval

Equation	Lags(6)	F-Ratio	P-Value (<)
Approval	approval	15.30	.001
Approval	monetary base	.74	.62
Approval	interest rate	.42	.86
Approval	unemployment	1.81	.10
Approval	prices	3.29	.006
Approval	federal expenditures	1.06	.40
Approval	real GNP	4.81	.0003
Monetary base	approval	3.15	.008
Interest rate	approval	.66	.68
Unemployment	approval	1.03	.41
Prices	approval	1.63	.15
Federal expenditures	approval	.51	.80
Real GNP	approval	2.55	.03

Note: Each entry is the F-Ratio for a test of the null hypothesis that all of the six coefficients of the lagged variable are equal to zero. The distribution under the null for each test is the F-distribution with (6,74) degrees of freedom.

pretation being that on average, presidents are more willing to propose contractionary policy responses when public approval is higher.

Evidence in favor of the importance of approval in accounting for the monetary base can be found in Table 3. The null hypothesis that all coefficients on approval lags are zero in the monetary base equation is rejected at very conservative probability levels. This "exogeneity" test is consistent with some structural equation specification of the monetary base, including approval lags on the right-hand side. It is well known that money is often found to be exogenous in relation to many macroeconomic variables, making causal priority of approval over money even more striking.

The response of money to an approval innovation should be negative, and MAR dynamic response analysis is used to assess these dynamics. However, MAR analysis requires an additional restriction that is not necessary for initial estimation. Some method must be used to partition the contemporaneous relationships among the variables or else the response will unrealistically ignore some of the contemporaneous correlations among the variables. Before partitioning, the contemporaneous relationships among the variables are described by the variance-covariance matrix of the disturbance terms, **R**. The conventional method for partitioning the covariances into contemporaneous relations is the Choleski decomposition, which produces a triangular matrix of contemporaneous effects. The Choleski method decomposes **R** into **S**, the matrix of contemporaneous effects, through solving the equation $\mathbf{CC'} = \mathbf{R}$, where **C** is lower triangular. This has the effect of allowing the first variable in the ordering to affect all other variables contemporaneously, allowing the second variable to influence all but the first, and so on. As long as the contemporaneous correlations among the disturbances are small, the MAR response is very robust to choices of ordering. If the correlations are large, however, the MAR responses will not be robust, as the first variable in an ordering between two highly correlated variables will account for more of the response of the second variable and other variables later in the ordering than would happen if the ordering were reversed.

Three different orderings were used to check for sensitivity of the MAR to the order of variables. Because estimating the response of other variables to an innovation in approval is the primary motivation of the MAR analysis, two triangular orderings—one with approval first in the ordering, another with approval last—were performed. In general, the response charts showed that choice of ordering does not produce appreciably different responses of variables to approval shocks. Thus, the first two triangular orderings are not presented here; and a third ordering, based on a more realistic relationship among contemporaneous innovations, is displayed.[28]

The third ordering is based on "quasi-structural" restrictions on the contemporaneous relationships among innovations, not on the more severe triangular restrictions. These overidentifying recursive restrictions allow for approval to influence policy variables contemporaneously, with macroeconomic outcomes influencing approval instantaneously. We know that innovations in approval without accompanying policy changes should not influence economic aggregates, and we can exploit this knowledge in specifying the contemporaneous relations. Using maximum likelihood methods, **C** is estimated by finding the best fit between an implied contemporaneous variance-covariance matrix **S** with the actual matrix **R**, such that $CC' = S$ (Sims 1986a).[29]

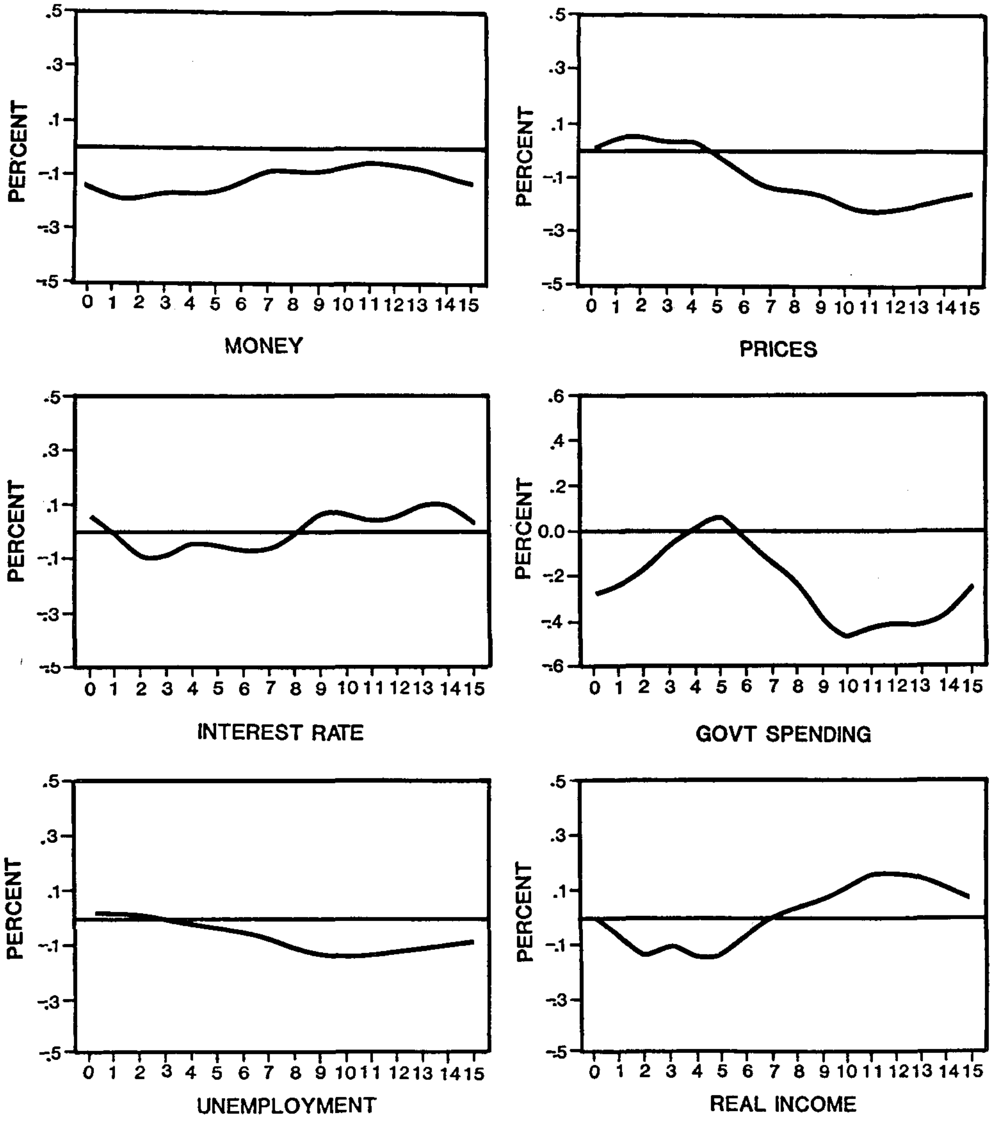
Figures 3 and 4 present many of the MARs from the VAR. Each response is a deviation from the forecasted values the variables would take if no shock entered the system. With no shock, the forecast is represented by the horizontal line at zero. The most significant response to an approval innovation is the decrease in the monetary base of almost .2% after a positive approval shock.

For the policy responses that are most relevant for the referendum model, confidence intervals (or posterior distributions) are calculated in order to assess the importance of the responses. If a response interval contains either all positive or all negative values, the response can be considered "significant" in a way similar to a classical confidence interval. A 90% posterior distribution calculated by the method suggested by Sims (1987) contains no positive values in the first three quarters for any of the causal orderings. The posterior distribution clearly centers on negative values of money.

Interest rate response, contrary to expectations, is negative, reaching about −.1%; but the posterior distribution contains both positive and negative values, so that any conclusion about interest rates is difficult to make. Real income is the only macroeconomic variable that also responds moderately to the approval innovation, suggesting that the monetary policy contraction associated with a positive—or expansion associated with a negative—approval innovation may indeed have real effects. Long-term response is more difficult to interpret because of the complicated feedback among variables. In addition, the posterior distributions of the MAR responses grow very large after six to eight quarters.

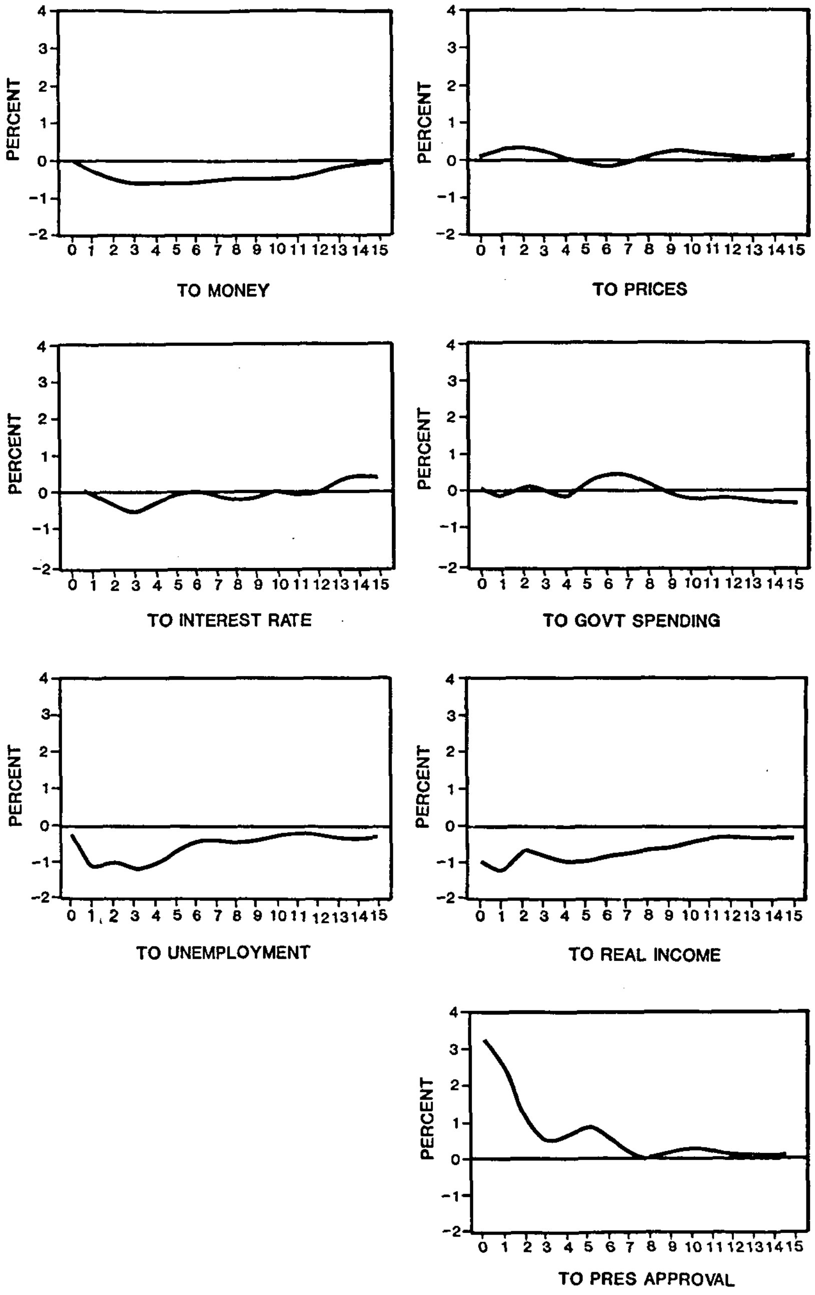
Table 4 contains the decomposition of forecast error, which is very useful for determining the importance of a variable's influence on another variable. Each entry represents the percentage of forecast error in one variable at a variety of time points that would be due to forecast errors in other variables. Large values indicate that one variable is an important predictor of another, and we would expect two related variables to have correlated forecast errors. Approval accounts for a relatively substantial percentage of the forecast error variance of money, more than any other macroeconomic variable (except money's own error variance) through five quarters. This relative size of influence is substantial. The importance of approval in accounting for other variables is minimal and probably the result of feedback in the system, including movements of the

Figure 3. Moving Average Responses of VAR with Quasi-structural Ordering of Variables: Response of System to a One-Standard-Deviation Shock in Approval



Note: This moving average representation is from the VAR described in the text. All responses are in percentages of the initial values of each variable. The contemporaneous causal structure for this moving average representation is described in n. 29.

Figure 4. Moving Average Responses of VAR with Quasi-structural Ordering of Variables: Response of Approval to One-Standard-Deviation Shocks in Money, Interest Rate, Unemployment, Prices, Government Spending, Real Income, and Approval



Note: This moving average representation is from the VAR described in the text. All responses are in percentages of the initial values of each variable. The contemporaneous causal structure for this moving average representation is described in n. 29.

Table 4. Decomposition of Forecast Error Variance for Quasi-Structural Vector Autoregression

	Innovation in						
Quarters Ahead	Approval	Monetary Base	Interest Rate	Unemployment	Prices	Federal Expenditures	Real GNP
Approval							
0	90.9	0.0	0.0	.8	.2	0.0	8.0
1	81.0	.2	.0	5.3	.4	4.1	8.9
2	80.0	.6	.1	5.5	.5	5.3	8.0
5	70.9	.9	.9	9.1	1.5	8.6	8.2
9	66.3	1.9	2.1	9.4	2.3	7.9	10.0
Monetary base							
0	6.3	76.4	1.9	0.0	11.2	1.1	3.1
1	11.1	72.8	2.0	1.6	8.8	.5	3.1
2	13.9	68.5	1.7	3.6	7.5	.4	4.5
5	14.1	48.1	12.6	13.0	4.1	.7	7.3
9	11.0	43.3	15.4	15.0	3.6	.8	10.9
Interest rate							
0	1.2	0.0	97.1	0.0	1.7	.0	0.0
1	.5	1.0	93.7	1.1	1.4	1.0	1.4
2	2.4	1.5	87.8	1.9	1.4	1.1	3.9
5	4.3	4.1	79.0	3.9	1.7	1.1	5.8
9	4.4	6.5	74.6	6.1	1.3	1.7	5.4
Unemployment							
0	0.0	.0	12.5	84.5	2.1	.0	.9
1	.2	.7	8.9	84.8	4.3	.6	.5
2	.4	1.8	4.9	84.7	6.7	.5	1.0
5	.4	7.1	16.8	57.4	12.9	2.2	3.2
9	2.7	14.0	31.8	34.9	8.8	1.1	6.8
Prices							
0	0.0	0.0	0.0	0.0	96.5	.0	3.5
1	1.8	.0	.0	.2	95.7	.1	2.1
2	3.2	.1	2.3	.6	91.5	.1	2.2
5	1.5	11.8	42.9	.4	37.0	1.0	1.2
9	2.3	24.7	33.8	2.7	18.6	3.8	5.2
Federal expenditures							
0	2.4	0.0	0.0	0.0	0.0	97.6	0.0
1	2.7	1.0	.8	3.8	.0	91.6	.2
2	2.4	1.2	.7	5.5	.6	86.4	3.0
5	1.6	3.4	3.4	4.7	6.2	76.0	4.8
9	3.3	5.2	8.0	4.5	5.8	61.8	11.3
Real GNP							
0	0.0	0.0	.0	0.0	0.0	.1	99.9
1	.9	.7	.8	.5	1.2	.3	95.6
2	3.9	2.0	8.2	4.6	4.2	.9	76.3
5	4.8	1.4	7.8	39.6	7.9	.8	37.8
9	3.1	4.7	19.2	35.2	9.8	3.7	24.2

Note: The cells in this table represent percentages of the forecast error in the row variable (*q* quarters ahead) that are induced by innovations in the column variables. Larger values indicate that one variable's forecast *q* quarters ahead depends on another variable's errors and thus that the decomposition of forecast error variance is useful for tracing a variable's influence throughout the dynamic model. 0.0 entries are the pairings that, given the contemporaneous structure, must be zero.

interest rate. Approval accounts for only a small proportion of the variance of interest rates.

One anomaly in the results centers on the failure of interest rates to respond in a contractionary manner to approval shocks. If money moves in a contractionary manner, why do interest rates remain relatively stable? This question is difficult to answer using the historical data, but some speculation is useful. Money and interest rates do move consistently over the election calendar (see Figure 1); thus, the difference may be a result of how the president responds to approval shocks. The MAR results show what happens when a typical, unexpected increase (decrease) in approval occurs. The Federal Reserve would have to respond with marginal changes in its operating instruments (primarily the federal funds rate), which should influence interest rates and the money supply. However, money supplied is also a function of demand in markets; and interest rates are determined in markets where future rates and prices are important determining factors. If the politics of the monetary policy change require some public discussion and presidential position to be taken (i.e., if the president wants to seem to be trying to improve the economy), some announcement of intention may be made, the result of which would be to change rates much more quickly than the monetary base will change. The MAR response chart shows that interest rates increase a small amount at the time of an approval innovation (about .6%), but rates quickly return to preinnovation levels and below. At the same time, the slight increase in future prices plus the expectations that rates will rise leads to lower demand for money, producing a longer-term response in money that is not mirrored in interest rate changes. A one-time policy response leads to a small change in the interest rate and a much longer-term decline in money.

This speculation about the relationships among approval, money, and interest rates is consistent with the short-term decline of real income (.15% of its original forecasted level) in response to an approval innovation. Decreasing growth rates of income can be a function of money growth decreases or interest rate increases, suggesting that the movement of policy variables to innovations in approval are, initially at least, connected to policy actions. Real income responds more strongly to interest rates than it does to money innovations (see Table 4); but some combination of money, price, and interest rate changes appears to account for relative real income decline.

The evidence strongly supports a model in which the president manipulates policy in the short run in reaction to innovations in his approval rating. Any specific model of macroeconomic policy behavior must account for the response of money to approval. However, the MAR analysis of the response of approval to innovations in the macroeconomic variables is more perplexing.[30] Figure 4 gives these MAR responses. Approval responds negatively to unemployment surprises as the public places blame on the president for economic downturns. Likewise, approval declines with an innovation in interest rates. However, the response of approval to real income innovations is negative, indicating that the public punishes a president for increased economic growth. This relationship reverses itself if petroleum prices are excluded from the model, suggesting that the supply shocks of the 1970s may have complicated the relationship between real growth and approval. These results are also understandable in light of the high correlation between unemployment and real income. And they do not indicate that a president can gain from a recession because the public responds strongly to unemployment, an economic indicator that receives much more attention in the press than does the growth rate. F-tests (Table 3) indicate that some combination of price, unemployment, and real income changes predict approval; but the interpretation of these effects is far from straightforward, as the relationship between real income and approval is in the wrong direction.

Figure 4 shows that public support for the president increases (decreases) sharply after a negative (positive) approval innovation, but it is difficult to sort out how much of this increase originates through political manipulation of the economy and how much is simply a reflection of the dynamics of presidential approval. This model of macroeconomic policy manipulation is consistent with the dynamics of approval, however, since policy actions taken by the president would cause approval to return relatively quickly after an innovation. However, it takes approval at least four years to return to preinnovation levels.

In summary, when a president faces decreasing approval ratings, he is more likely to favor and deliver expansionary monetary policies (as is the Federal Reserve). This policy response is reflected in changes in money, but limited evidence suggests that interest rates decline contemporaneously if they respond at all. However, the increase in the growth of real income in response to a negative approval innovation indicates that this policy response influences the macroeconomy, although the dynamics between real income and approval imply that this increase may not help the president overcome the initial drop in popularity.

Conclusion

The primary goal of this analysis has been to test a variety of models of macroeconomic policy choice using VAR. It bears repeating that much of the VAR analysis I report lacks precision in the estimates. The philosophy of VAR analysis is based on the idea that a VAR gives a realistic picture of uncertainty and that conventional classical approaches to inference, in practice, underestimate actual uncertainty. One consequence of this difference in approach is that many of my findings will not hold up under classical hypothesis testing procedures. However, I have reported a number of tests to guide the reader. Much reported here will change in different samples, although some attempt was made to find important dynamics that are robust to alternative samples.[31] In short, most attention should be given the large effects; and smaller effects should be treated as if they are much less likely to generalize to other samples.

Evidence in support of the party differences model is mixed, although many of the findings are relatively consistent with recent research building on Hibbs's analysis. However, the finding that unemployment rates have not differed by party is in significant contrast with much of the literature.

The results are consistent with a political monetary cycle, as interest rates are more expansionary as a result of elections. However, with the exception of real income, other macroeconomic outcomes do not exhibit the behavior described by a classical political business cycle. Most importantly, the cost of higher inflation as a result of an electoral cycle in interest rates is not reflected in the data. In general, the data suggest that elections enhance macroeconomic performance.

The short-term reaction of money to approval shocks is consistent with a model in which the president uses monetary policy to try and manage public support. More typical countercyclical behavior of the macroeconomy follows these short-term policy responses as expansionary movement in policy resulting from decreasing public support leads to increasing relative economic growth. Indeed, any cycle in economic performance resulting from political manipulation may best be described as a combination of short-term reactions of the president coupled with long-term planning for elections. The monthly Gallup polling of the U.S. public gives the president a continual referendum in which to base changes in policy. Once this dynamic is taken into account, elections themselves are quite benign. It is the continuing referenda that place pressure on presidents.

The most remarkable finding is the benevolent role of elections in shaping macroeconomic policy. The classic political business cycle as described by Nordhaus (1975), Tufte (1978), and Chappell and Keech (1983) all present views of the political business cycle that imply that macroeconomic policy would be better served if democratic elections did not interfere with technocratic policy formation and implementation. The results of this analysis do not support these views.

No extant theory explains these VAR results on elections, but speculation is possible. Macroeconomic policy choice is technical, but it is not a technocratic policy capable of producing accurate policy forecasts. Too many different and extremely divergent models of macroeconomic policy exist, and policy guidance requires more than the selection of the right tools. Elections may actually ensure that doctrinaire macroeconomic policy analysts do not get control of the policy apparatus because politicians will be afraid of theory that does not work. The move to monetarism in 1979 became tempered by elections in 1982, and elections can be seen as a check on policy experimentation by theorists who may or may not have the correct model of the macroeconomy. Left to their own devices, with no elections, these theorists could ruin an economy before deciding the policy experiment is a failure. With elections, practical politics supersedes these possibilities. The Fed has always remained practical even if it does in theory experiment with interest rate and money growth targets. Neither of these targets has ever been focused on so potently as to blind the Fed to other indicators of policy direction (Woolley 1984). This is one possible model worthy of investigation.

There are other possible explanations for these results on elections and macroeconomics. Some rational expectations model may be consistent with the VAR results, one in which voters do not blindly support expansionary policies without an eye toward the future. However, any model of this type must be able to explain the electoral cycle in interest rates as well as short-term responses of money to approval innovations. Of course, politicians may follow standard operating procedures, conventional wisdom being that expansion means votes and popularity. If voters are sophisticated and economic agents anticipate conventional political actions by the president, these political actions may be negated. Indeed, short-term manipulation appears consistent with such a model in that policy manipulation does not appear to improve the president's standing in any appreciable way.

Just how short-term manipulation of the economy fits with the electoral results is unclear. Perhaps in the short run, monitoring of public opinion by the president leads to very conventional strategies for opinion management that lead the president to support more expansionary monetary policies when approval is low. The month-to-month policy changes reflect this political convention, and elections function to keep policy in the mainstream, as speculated above. Thus, elections do have the hypothesized effects on the macroeconomy—but only because they make public opinion important to politicians.

Perhaps one day, if all macroeconomic theorists agree on the correct prescription for dealing with macroeconomic problems, elections and presidential politics will conspire to undermine sound economic planning. As of now, the political manipulation of macroeconomic policy does not look so bad, as a benevolent despot capable of pulling the "correct" levers might inadvertently produce inferior policies. For now, maybe democracy works because technical knowledge about macroeconomics is uncertain.

This optimistic view of democracy is merely speculation at this point, but it serves as a satisfying contrast to the large volume of research in public choice critical of democracy. Perhaps this picture of democracy as a check on the evils of technocracy is worth more thought, seeing how one of the most extreme examples of technocracy, the Soviet Union, is currently moving toward a more democratic model of governing. Democratic politics may offer a unique alternative, one in which technocracy is held in check. However, public choice theorists have to this point failed to view democracy in this way; and they thereby condemn public sector activities as "inappropriate." In terms of macroeconomic policy, politics as *second best* looks better than many other possibilities; and public choice theorists should appreciate the possibility that democratic political institutions can in some circumstances lead to better policy responses.

Notes

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1. Aranson and Ordeshook (1985, 87) maintain, "The outlines of consensus have emerged concerning the appropriateness of public-sector activities in representative democracies. The consensus holds that most public-sector programs are inappropriate, or are carried on at an inappropriate level, or are executed in an inappropriate manner."

2. Equally important is how presidents try to influence fiscal policy. In the 1980s, taxes, spending, and deficits have been key issues in presidential elections. I analyze federal expenditures; but at the macrolevel, too much aggregation makes serious analysis of election-oriented spending difficult if not impossible. Tufte (1978) and Kiewiet and McCubbins (1985) focus on spending and elections; and Winters and colleagues (1981) perform a detailed analysis of election-year spending priorities, an analysis that serves as a testament to the difficulty of sorting out electorally motivated spending patterns.

3. Beck (1982b) presents some evidence that the differences between parties may be less significant than that found by Hibbs. Furthermore, Beck (1982a, 1984) casts doubt whether monetary policies are best explained by differences in presidential administration, leading to a tentative conclusion that Hibbs's result could well be a function of nonpolicy factors. Others (Alesina and Sachs 1988; Chappell and Keech 1988a, 1988b) also find that when policy mechanisms for generating differences in unemployment rates are accounted for, estimates of party differences are much smaller than those Hibbs finds. Woolley (1988) shows how average party differences may be small even if there are significant partisan effects. Instability across time, due to changes in the economic environment and macroeconomic technology, may result in negative findings even though differences in administrations can be given partisan interpretations.

4. These are by no means the first works dealing with the hypothesis that macroeconomic variables may exhibit cyclical properties as a result of electorally motivated policies. Wright (1974), for one, investigates a model based on Downsian spatial voting that explains some movement in New Deal policy innovation.

5. The common assumption behind this model—as well as the electoral cycle model—is that tight money and contractionary policies will always be unpopular. While probably untrue for all cases, the conventional wisdom among policy makers is that the public is not tolerant of the short-term costs of tight money; and policy makers believe this convention. For a discussion of this core assumption, see Alt and Chrystal 1983, chap. 5.

6. The most blatant example of the failure to formally estimate the importance of feedback can be found in Golden and Poterba 1980, where a popularity function is estimated and then treated as fixed for the purposes of evaluating the amount of economic manipulation needed for the president to gain a point of popularity. In essence, Golden and Poterba estimate a system without feedback and then use the same model to evaluate the use of feedback. The referendum model requires the estimation of feedback from presidential approval to economic instruments.

7. By *conventional* I mean two things: (1) structural, in that the model's coefficients represent direct relationships among variables; (2) parsimonious, in that conventional models usually require a small number of parameters to be estimated in comparison

to the error degrees of freedom of the regression. Parsimonious reduced-form models are now more common because they overcome some of the problems associated with structural model building.

8. Wold 1938 is an obvious exception, and it is not a coincidence that his views about dynamics have recently become very important in economics. Lucas (1981, intro.) sees the movement to rational expectations models as a result of repeated attempts to refine static Keynesian macroeconomics by moving to a dynamic representation of the macroeconomy.

9. Sargent (1981) outlines the implications of the rational expectations research program for econometric practice. Many of his criticisms are consistent with mine.

10. This third source of dynamics is typically investigated using reaction functions (Alt and Chrystal 1983, chap. 6). The difficulties with using historical data to estimate policy preferences of policy makers and aggregate supply-and-demand functions simultaneously are well understood (Chow 1984, chap. 12). The expectations of policy makers also serve to complicate dynamics. Policy is usually a response to innovations in macroeconomic outcomes, as predictable movements in target variables will not require policy changes. In addition, policy can be defined as innovation. This view of monetary policy is now commonplace, though the exact meaning of a policy innovation remains a source of controversy. Barro (1977) presents the classical rational expectations view that defines innovations in the money supply as the objectively unpredictable parts of money or, equivalently, as errors in reaction functions. If the Fed is capable of forecasting future market changes better than market participants, some predictable components of money may still qualify as innovations. Of course, the president may respond to anticipated movements in his approval rating, thus resulting in further complication of data dynamics.

11. The role of policy preferences in guiding choices of political officials has been a source of speculation by those uncomfortable with the Downsian assumption that leaders make policy merely to get elected or reelected, rather than getting elected to make policy. Most notably, Wittman (1983) and Chappell and Keech (1986) investigate spatial models including politicians with policy preferences.

12. Zellner (1984, chap. 1.3), among others, is skeptical of the method and outlines some disadvantages of using unrestricted dynamic models for data analysis.

13. Ordinary least squares produces consistent estimates as long as the residuals are white noise and the same variables appear in each equation (Theil 1971, 309–10).

14. Granger causality does not necessarily mean *causality* in the sense in which philosophers use the term. Rather, the concept refers to causal priority in a strictly statistical sense (see Sims 1977). Causal priority in time series data can result from processes that are not consistent with philosophical conceptions of causality; thus, the term *Granger causality* is best thought of as a statistical characteristic of a pair of variables.

15. Real GNP, prices, federal expenditures, and the monetary base are all in natural logs. All variables except approval and expenditures are seasonally adjusted. Economic variables are from the Citibase data set, and presidential approval scores are from Gallup. The likelihood ratio test statistic described in Sims 1980 and Freeman, Williams, and Lin 1989 determined that six lags were needed to approximate the VAR. The approval equation includes the restriction that economic conditions in one president's term do not influence the approval rating of the succeeding president. Trial and error showed no appreciable difference in models using M1 in place of the monetary base and the consumer price index in place of the GNP deflator, especially in terms of the conclusions I reach. Using the monetary base allows the analysis to begin in 1953 without needing to splice old and new M1 measures together.

16. I have excluded a war dummy of the type Hibbs (e.g., 1987a, 247) uses because its effect on the dynamics of the system were minimal.

17. Alesina (1988) attempts to overcome this problem by specifying a symmetry in the dummy variable where the first quarters of a term are given negative values.

18. In other words, elections are by nature discontinuous events, and the specification of the election trend variables allows for this natural discontinuity. Macroeconomic variables are highly serially correlated and thus are continuous, and the VAR accounts for as much of this continuity as possible. Combining the election trend terms with the VAR allows for these disparate sources of variance to be accounted for simultaneously.

19. Chappell and Keech 1988a and 1988b, Alesina 1988, and Alesina and Sachs 1988 all use different structural models and find differences in a host of economic outputs and policy instruments. Hibbs finds differences within the context of nonstructural models.

20. It is also important to note that statistical tests in this case—like party differences tests—are not particularly powerful. The simulations may uncover substantively important cycles that are not statistically significant. The actual coefficients are not reported here because the election effect is the result of two election trend terms and the presence of feedback makes interpretation of the estimates misleading—even more in this case than with the party coefficients.

21. The values on the left-hand axis are not meaningful in themselves but reflect the initial starting values, which are simply the values of the variables at the end of the sample period. The starting values will in part determine the magnitudes of differences.

However, the differences are realistic 1980s values because the starting values came from 1984. Variables not already scaled in percentages are presented in percentage increases over the initial values.

22. The statistical significance of the election trend terms in the interest rate equation implies that much of the difference in money over this simulation may be due to interest rate changes. The standard errors of the simulation indicate that the difference between the election and nonelection simulated values of both variables is approximately one standard error unit—not a large difference. This suggests that there is some relatively strong feedback from interest rates to some other variables and back to interest rates, since the election terms are significant in that equation.

23. The fact that prices are lower during the election period makes the difference in the monetary base and interest rates (Figure 1) even more important. Real money growth and interest rate differences will necessarily be larger than the nominal differences shown in Figure 1.

24. This large difference is unprecedented in the literature; it constitutes a sizable 2 1/2-standard-error contrast between simulated values.

25. I find this typical pattern for real income but not prices, as does Grier.

26. Beck's argument represents the conventional wisdom about monetary policy (for example, see Allen 1986).

27. Sims (1986b) argues that conventional parsimonious parameterizations of econometric models leads to an underestimation of the amount of uncertainty in our models. This underestimation is exaggerated by the specification searches most analysts use to find the final model. One consequence is that actual forecasts are almost always less precise than econometricians estimate them to be.

28. Each set of response charts not appearing in the text are available from the author.

29. The restrictions on C are given in the following pattern matrix:

Row	*Variable*							
1	approval	1	0	0	1	1	0	1
2	money	1	1	1	0	1	1	1
3	interest	1	0	1	0	1	1	0
4	unemployment	0	1	1	1	1	1	1
5	prices	0	0	0	0	1	1	1
6	expend	1	0	0	0	0	1	0
7	GNP	0	0	1	0	0	1	1

The row number corresponds to the column number for each variable, and each zero represents the restriction that the column variable does not contemporaneously influence the row variable. For example, in this contemporaneous structure, unemployment, prices, and real income all influence approval; and approval contemporaneously affects money, interest rates, and expenditures. The most crucial restriction is that interest rate is ordered before money, though each is best thought of as being determined simultaneously. However, the findings I discuss are robust to this ordering assumption.

30. The approval function is the least well specified of the equations, as many political variables capable of explaining presidential approval are excluded from the model. The purpose of estimation of the VAR model was not to have a well-specified approval equation but rather to allow feedback from approval to economic variables. Thus, one limitation of this analysis is in specification of the approval function; and some of the results may be a function of the specification.

31. Two robustness checks are important. Dropping the Eisenhower administration did not change the direction of important findings, and ending the analysis prior to the move to monetary targeting in 1979 did not significantly alter the response of money to approval.

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BUYING TIME: MONEYED INTERESTS AND THE MOBILIZATION OF BIAS IN CONGRESSIONAL COMMITTEES

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Over the last two decades institutional critics have increasingly charged that moneyed interests dominate the legislative process in Congress. Systematic research on campaign contributions and members' floor voting, however, provides little supporting evidence. We develop a view of the member-donor relationship that questions the theoretical underpinnings of the vote-buying hypothesis itself and suggests two alternative claims: (1) the effects of group expenditures are more likely to appear in committee than on the floor; and (2) the behavior most likely to be affected is members' legislative involvement, not their votes. In order to test this account, we specify a model of committee participation and estimate it using data from three House committees. In contrast to the substantial literature on contributions and roll calls, our analysis provides solid support for the importance of moneyed interests in the legislative process. We also find evidence that members are more responsive to organized business interests within their districts than to unorganized voters even when voters have strong preferences and the issue at stake is salient. Such findings suggest several important implications for our understanding of political money, interest groups, and the representativeness of legislative deliberations.

At least since Madison railed about the mischiefs of faction, critics of U.S. political institutions have worried about the influence of organized interests in national policy making. In this century, one of the most eloquent critics of the interest group system was E. E. Schattschneider, who warned of the inequalities between private, organized, and upper-class groups on the one hand and public, unorganized, and lower-class groups on the other. The pressure system, he argued in *The Semisovereign People* (1960), "mobilized bias" in national policy making in favor of the former, against the interests of the latter, and hence against the interests of U.S. democracy. Such concerns have hardly abated thirty years since the publication of Schattschneider's essay. In particular, the precipitous growth in the number and financial strength of political action committees has refueled the charge that moneyed interests dominate the policy making process. The current Congress is *The Best Congress Money Can Buy* according to one critic (Stern 1988), one where *Honest Graft* is an institutional imperative (Jackson 1988; see also Drew 1982; Etzioni 1984). "The rising tide of special-interest money," one close observer concludes, "is changing the balance of power between voters and

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donors, between lawmakers' constitutional constituents and their cash constituents" (Jackson 1988, 107).

Despite the claims of the institutional critics and the growing public concern over PACs during the last decade, the scientific evidence that political money matters in legislative decision making is surprisingly weak. Considerable research on members' voting decisions offers little support for the popular view that PAC money permits interests to buy or rent votes on matters that affect them. Based on an examination of 120 PACs in 10 issue areas over four congresses, one recent study concludes flatly that PAC contributions do not affect members' voting patterns (Grenzke 1989a). Another study, designed to explore the "upper bounds" of PAC influence on House roll calls, emphasizes "the relative inability of PACs to determine congressional voting" (Wright 1985, 412). Other studies have come to similar conclusions (see e.g., Chappell 1982; Wayman 1985; Welch 1982), though there are also dissenting voices (e.g., Kau and Rubin 1982; Silberman and Durden 1976). On the whole, then, this literature certainly leads one to a more sanguine view of moneyed interests and congressional politics than one gets from the popular commentaries. Does money matter?

Our approach to this question is two-pronged. In the first two sections, we revisit the question by developing a theoretical account of the constrained exchange between legislator and donor quite different from the one evident in the substantial literature cited above. In particular, we adopt the premise that PACs are rational actors, seeking to maximize their influence on the legislative outcomes that affect their affiliates; but we take issue with the standard account of PAC rationality. Our approach does not lead us to predict a strong causal relationship between PAC money and floor votes. House members and interest group representatives are viewed as parties to an implicit cooperative agreement, but the constraints on member behavior and the rational calculations of group representatives limit the extent to which votes become the currency of exchange. Instead, we advance two hypotheses about the effect of money on congressional decision making.

First, we suggest that in looking for the effects of money in Congress, one must look more to the politics of committee decision making than those of the floor. This view, of course, is neither original nor remarkable. Students of Congress have long contended that interest group influence flourishes at the committee level, and recent students of PAC influence invariably advocate that work move in this direction (e.g., Grenzke 1989a, 18; Schlozman and Tierney 1986, 256). To date, however, systematic studies of PACs and committee decision making have been altogether rare (for an important exception, see Wright 1989). We focus here at the committee level and emphasize the theoretical reasons for doing so.

Second, and more importantly, our account of the member-donor exchange leads us to focus on the *participation* of particular members, not on their votes. This variable, we believe, is a crucial but largely neglected element of congressional decision making. It is especially important in any analysis of interest group influence in a decentralized Congress. In their famous study of lobbying on foreign trade policy, for instance, Bauer, Pool, and Dexter concluded that a member's principal problem is "not how to vote but what to do with his time, how to allocate his resources, and where to put his energy" (1963, 405). More recently, Denzau and Munger (1986) have modeled the interest group–member relationship as an exchange of contributions and electoral support for legislative services or effort. If money does not necessarily buy votes or change minds, in other words, it can buy members' time. The intended effect is to mobilize bias in congressional committee decision making.

We then develop and estimate a model of committee participation that permits a direct test of whether moneyed interests do mobilize bias in committee decision making. Analyzing data from three House committees on three distinct issues, we find that they do. In the final section we briefly discuss the implications of the findings for our understanding of money, interest groups, and representation in Congress.

The Rational PAC Revisited

The interdependencies of legislators and moneyed interests have been widely discussed by political scientists and widely lamented by critics of pluralism (see esp. Hayes 1981). The basis for political exchange is clear. Each depends at least partially on the other to promote its goals. Interest groups seek, among other things, favorable action on legislation that will affect them; members of Congress seek financial and political support from particular groups. Like the relationship between legislators and bureaucrats, however, the relationship between legislators and interest groups is one of *implicit* exchange: the actors "trade speculatively and on credit" (Arnold 1979, 36; see also Denzau and Munger 1986; Hayes 1981). Contributions are marked somewhere in the invisible ledger, and a group's political strategists presumably can use them to their momentary legislative ends.

This account of the legislator–interest group relationship underpins the now-considerable literature on contributions and roll call voting. The working hypothesis is that contributions influence legislative outcomes by "purchasing" the votes of particular members or, less directly, by serving as "investments" that will pay dividends in legislative support at some later date (e.g., Chappell 1982; Jacobson 1980, 77, 82). The scientific evidence that such effects appear only infrequently may be cause for relief among critics of the system, but it is puzzling to theorists of institutional behavior. Why should PACs flourish, both in number and financial strength, when their legislative efficacy is so low? The payoffs would appear inadequate to sustain the cooperative relationship.

One possible explanation is that PACs raise and disburse money with local congressional elections, not specific legislative ends, in mind. Wright (1985) argues, in fact, that the decentralized nature of most PAC organizations inclines them to do just that. But this account simply moves the issue of PAC rationality to a second, institutional level. Why would PACs organize in this way? Wright suggests that the typical national PAC office permits local officials substantial discretion because it wants to encourage them to continue raising funds. But the organization's fund-raising and disbursement, presumably, are intended for some more ultimate purpose, namely, to increase the net political benefits associated with governmental action (or inaction) on issues that affect it. On the whole, using money solely to affect election outcomes is not likely to be a rational means to this end. The probability that any single group's contribution will affect the outcome of a congressional election—in which a wide range of more powerful forces are at work—is almost certainly slight. In the aggregate it might affect the organization's political support within Congress by only a member or two (Wayman 1985). While organizational arrangements may create some inefficiency in the way PACs employ funds to promote their political ends, one should still expect to find systematic patterns of allocation that are driven by legislative considerations, even among PACs that are highly decentralized (and especially among those that are not). Indeed, there is growing evidence that this is the case (Grenzke 1989b).

If the principal value of contributions lies in their potential to affect floor roll calls, however, a second puzzle appears. One would expect to find contribution strategies that favor the swing legislators in anticipated floor battles, since these are the cases where the marginal utility in votes purchased per dollar spent is likely to be greatest (Denzau and Munger 1986). Money allocated to almost certain supporters (or almost certain opponents) should be counted as irrational behavior, evidence of scarce resources wasted. In fact, however, the evidence suggests that such "misallocations" systematically occur. The Business-Industry Political Action Committee (BIPAC) and the National Chamber of Commerce give overwhelmingly to conservative Republicans (Kau and Rubin 1982, 88; Maitland 1985). Labor PACs such as the AFL-CIO's Committee on Political Education give overwhelmingly to incumbent Democrats loyal to labor's agenda (Chappell 1982; Grier and Munger 1986; Jacobson 1980). Oil PACs give to conservative incumbents regardless of party and to friends regardless of ideology (Evans 1988). In general, PACs are prone to reward their friends—even when their friends are not in danger of defeat. In a specific test of the swing hypothesis, in fact, Welch found that if anything, dairy PACs were *less* likely to contribute to swing legislators on dairy issues, all other things being equal (1982).[1] On the whole, it would seem that if, as Schattschneider (1960) said, moneyed interests sing with an upper-class accent, they also spend a good deal of effort singing to the choir.

One oft-mentioned solution to these puzzles is that contributions buy not votes but "access" to members and their staffs (e.g., Berry 1984; Gopoian 1984; Schlozman and Tierney, 1986). But this solution only provokes a second query: If money buys access, what does access buy? (see esp. Herndon, 1982, 1017). Presumably, it gives the representatives of contributing groups important opportunities to directly lobby and potentially persuade legislators to the group's point of view. In this scenario the language of *access* may serve symbolically to launder the money going from group to roll call vote, but the effect of the group on the vote should still appear in systematic analysis (Grenzke 1989a). As we note above, it does not.

The Rational PAC Revised

The literature on PAC contribution strategies and members' roll call voting behavior thus suggests two puzzles. First, if group strategists are reasonably rational, why would they continue to allocate scarce resources to efforts where the expected political benefits are so low? Second, if PAC allocation strategies are designed to influence members' votes, why do they contribute so heavily to their strongest supporters and occasionally to their strongest opponents? Is it the case that PACs are systematically irrational (e.g., Welch 1982, 492) and, by extension, that claims about the influence of money on legislative process almost certainly exaggerated? We believe that the premise of rationality need not be rejected but that theoretical work in this area requires a more complete account of rational PAC behavior. We extend here an account developed formally in Denzau and Munger's model of a supply price for public policy (1986). Simply put, interest group resources are intended to accomplish something different from, and more than, influencing elections or buying votes. Specifically, we argue that PAC money should be allocated in order to *mobilize* legislative support and *demobilize* opposition, particularly at the most important points in the legislative process.

This argument turns directly on what we already know about the nature of legislators' voting decisions from a very rich literature. The simple but important point is that a number of powerful factors exist that predispose a member to vote a certain way, among them party leaders, ideology, constituency, and the position of the administration (Fiorina 1974; Jackson 1974; Kingdon 1981).[2] Kingdon notes, moreover, that members' votes on particular issues are also constrained by their past voting histories (1981, 274–78). Members attach some value to consistency, independent of the other factors that influence their voting behavior. A third and related point is that the public, recorded nature of the vote may itself limit the member's discretion: a risk-averse member may fear the appearance of impropriety in supporting major campaign contributors in the absence of some other, legitimate force pushing her in the same direction. Finally, the dichotomous nature of the vote acts as a constraint. Money must not only affect members' attitudes at the margin but do so enough to push them over the threshold between *nay* and *yea*. In short, the limits on member responsiveness to messages wrapped in money are substantial, perhaps overwhelming, at least insofar as floor voting is concerned.

Of course, almost all studies of PAC contributions and roll calls acknowledge the importance of such factors and build them into their statistical models of the voting decision. But it is also important to consider the implications of these findings for the vote-buying hypothesis itself. Interest group strategists tend to be astute-enough observers of the legislative process to appreciate the powerful constraints that shape members' voting behavior. To the extent that this is true the rational PAC should expect little in the way of marginal benefits in votes bought for dollars spent, especially when individual PAC contributions are limited by the Federal Election Campaign Act to ten thousand dollars—a slight fraction of the cost of the average House race. Individual votes, that is, simply aren't easy to change; and even if some are changed, the utility of the votes purchased depends on their net cumulative effect in turning a potentially losing coalition into a winning one. For the rational PAC manager, the expected marginal utility approximates zero in most every case. All other things being equal, scarce resources should be allocated heavily elsewhere and to other purposes.

How, then, should the strategic PAC distribute its resources? The first principle derives from the larger literature on interest group influence in Congress. Well aware of the decentralized nature of congressional decision making, interest groups recognize that resources allocated at the committee stage are more efficiently spent (e.g., Berry 1984; Grier and Munger 1986; Kingdon 1981, 170–71). Interest group preferences incorporated there have a strong chance of surviving as the bill moves through subsequent stages in the sequence, while provisions not in the committee vehicle are difficult to attach later. Second, the nature of the committee assignment process increases the probability that organized interests will find a sympathetic audience at the committee or subcommittee stage. Members seek and often receive positions that will permit them to promote the interests that, in turn, help them to get reelected (Shepsle 1978). Finally, the less public, often informal nature of committee decision making suggests that members' responsiveness to campaign donors will receive less scrutiny. Indeed, a long tradition of research on subgovernments emphasizes that such clientelism flourishes at the committee stage (e.g., Ripley and Franklin 1980; Shepsle 1978, chap. 10; but see Gais, Peterson, and Walker 1984). In short, groups will strategically allocate their resources with the knowledge that investments in the politics of the appropriate committee or subcommittee are likely to pay higher dividends than investments made elsewhere. Indeed, this principle is especially important in the House, where the sheer size of the chamber's membership, the greater importance of the committee stage, and the frequent restrictions on floor participation recommend a more targeted strategy (see, esp., Grenzke 1989b and Grier and Munger 1989).

If PACs concentrate at the committee level, what, specifically, do they hope to gain there? Purchasing votes is one possibility; and, in fact, the rationale for allocating campaign money to buy votes in committee is somewhat stronger than for vote-buying on the floor. But even within committee, PACs still tend to give to their strongest supporters. In addition, committee votes, like floor votes, are dichotomous decisions. And despite the lower visibility of committee decision making, the factors of constituency, ideology, party, and administration are almost certainly at work. In fact, while research on PACs and committee voting is just now beginning to emerge, there is little evidence that contributions influence voting in committee any more than they do voting on the floor (Wright 1989).

The alternative hypothesis that we test here is that political money alters members' patterns of legislative involvement, a point that emerges from an older literature on interest group influence in Congress (e.g., Bauer, Pool, and Dexter 1963; Matthews [1960] 1973, esp. 192–93) but is given its fullest theoretical expression in the recent work of Denzau and Munger (1986). Denzau and Munger suggest that interest groups provide political resources in an implicit effort to purchase policy-relevant "services" from members or their staffs. Stated somewhat differently, the object of a rational PAC allocation strategy is not simply the *direction* of legislators' preferences but the *vigor* with which those preferences are promoted in the decision making process. Such strategies should take the form of inducing sympathetic members to get actively involved in a variety of activities that directly affect the shape of committee legislation: authoring or blocking a legislative vehicle; negotiating compromises behind the scenes, especially at the staff level; offering friendly amendments or actively opposing unfriendly ones; lobbying colleagues; planning strategy; and last and sometimes least, showing up to vote in favor of the interest group's position. The purposes of PACs in allocating selective benefits, then, are analogous to the purposes that Arnold attributes to legislatively strategic bureaucrats: the goal is not simply to purchase support but to provide incentives for supporters to act as agents—at the extreme, to serve as "coalition leaders" on the principal's behalf (see Arnold 1979, 40–42 and esp. 98–100).

Several arguments support this view. First, participation is crucial to determining legislative outcomes; and voting is perhaps the least important of the various ways in which committee members participate (Hall 1989; Mayhew 1974, 95). Second, while members' voting choices are highly constrained, how they allocate their time, staff, and political capital is much more discretionary (Bauer, Pool, and Dexter 1963, 406–7). At any given moment, each member confronts a wide range of opportunities and demands, the response to any subset of which will serve one or more professional goals. To be sure, the member must choose among them. Legislative resources are scarce, and their allocation to one activity results in other beneficial opportunities foregone (Bauer, Pool, and Dexter 1963; Hall 1987; Matthews [1960] 1973, 182–93). But for the most part, the purposive legislator is free to choose among the abundant alternatives with only modest constraints imposed by constituents, colleagues, or other actors. Hence, the member's level of involvement is something that a strategic PAC can reasonably expect to affect. The contribution need not weigh so heavily in a member's mind that it changes his or her position in any material way; it need only weigh heavily enough to command some increment of legislative resources. The minimum threshold that must be passed is thus a fairly modest one, and the potential effect of contributions on behavior is one of degree. Specifically, the member will allocate scarce legislative resources on the group's behalf so long as the marginal utility of the contribution to the member exceeds the expected marginal utility of the most valuable remaining use of the member's resources (see also Denzau and Munger 1986).

A third advantage of this view is that it explains the ostensibly anomalous tendency of PACs to contribute so heavily to members who are almost certain to win reelection and almost certain to support the group's point of view. Such behavior now appears quite rational. It is precisely one's supporters that one wants to mobilize: the more likely certain members are to support the group, the more active it should want them to be. Furthermore, this view of purposive PACs makes sense of the evidence that PACs sometimes contribute to members who will almost certainly oppose them and whose involvement in an issue stands to do the group harm. The PAC may have no hope of changing the opponent's mind, but it may, at the margin at least, diminish the intensity with which the member pursues policies that the organization does not like. The intent of the money, then, is not persuasion but demobilization: "We know you can't support us, but please don't actively oppose us." However, we should not expect the demobilizing effect of money to be nearly so strong as the mobilizing effect. The message provided through contributions to one's supporters is widely perceived as a legitimate one: in asking for help, the group is encouraging members to do precisely what they would do were resources plentiful. In contrast, contributions to opponents are meant to encourage them to go against their predispositions: the implicit message is to "take a walk" on an issue that they may care about. In short, the expected effects are not symmetric; the mobilization hypothesis is on stronger theoretical ground.

A final advantage of the view of rational action employed here is that it renders the matter of access more comprehensible. We have already noted that according to the standard account of PAC behavior, the importance that both legislators and lobbyists attach to the money-access connection makes little sense, given the evidence that money has little ultimate effect on votes. In light of the theory sketched here, however, access becomes an important, proximate goal of the interest group pursuing a legislative agenda. Access is central to stimulating agency. It gives the group the opportunity to let otherwise sympathetic members (and their staffs) know that some issue or upcoming activity is important to them. The ideal response they seek is not simply "I'll support you on this" but "What can I do to help?" Perhaps more importantly, access refers to the reciprocal efforts of the group. It is the pipeline through which the group effectively subsidizes the considerable time and information costs associated with their supporters' participation in the matters the group cares about. As various accounts reveal, group representatives often serve as "service bureaus" or adjuncts to congressional staff (e.g., Bauer, Pool, and Dexter 1963, chap. 24; Kingdon 1981, 154–55). They provide technical information and policy analysis; they provide political intelligence; they draft legislation and craft amendments; they even write speeches or talking points that their supporters can employ in efforts on their behalf. Such subsidies to the "congressman-as-enterprise" (Salisbury and Shepsle 1981) do not necessarily persuade, but they should affect the patterns of activity and abdication that have a direct bearing on legislative deliberations and outcomes (Hall 1987, 1989).

The Data: Money and Mobilization on Three Committees

The data for this investigation are drawn from staff interviews and markup records of three House committees on three issues: (1) the Dairy Stabilization Act, considered by the Agriculture Committee in 1982; (2) the Job Training Partnership Act (JTPA), considered by Education and Labor in 1982; and (3) the Natural Gas Market Policy Act, considered by Energy and Commerce during 1983–84.

Several features of these cases make them particularly appropriate for exploring the effects of money on the participation of committee members. First, all were highly significant pieces of legislation, the stakes of each measuring in the billions of dollars. At issue in the Natural Gas Market Policy Act was the deregulation of natural gas prices, a proposal that would transfer billions of dollars from one region to another, from consumer to industry, and within the industry from interstate pipelines and distributors to the major natural gas producers (Uslaner 1989, chap. 5; Maraniss 1983). Annual spending on the Job Training Partnership Act was expected at the time of its passage to be in the four-to-five-billion-dollar range (Donnelly 1982, 1035), and it replaced one of the most important domestic programs of the 1970s (Franklin and Ripley 1984). While more narrow than these in scope, the Dairy Stabilization Act also entailed significant economic effects. The principal purpose of the act was to adjust the scheduled support price for milk downward by as much as a dollar per hundredweight over two years, creating budget savings of 4.2 billion dollars for fiscal years 1983–85 and decreasing the profitability of milk production by as much as 30% for the typical dairy farmer. In each case, then, evidence of the influence of PAC money on congressional decision making can hardly be counted narrow or trivial. The deliberations in each case bore in significant ways on major interests, both public and private.

A second feature relevant to this investigation follows from the economic importance attached to these issues. All three were salient among actors other than the private groups immediately affected, a feature that the considerable research on roll call voting suggests should depress the effect of PAC contributions on congressional decision making (see, esp., Evans 1986). This was especially true for the natural gas and job training bills. While the Natural Gas Market Policy Act never received action on the House floor in the 98th Congress, it was a highly visible issue while still in committee. Consumer interest in the issue of natural gas pricing was unusually high. Gas heating costs had been climbing quickly in much of the country despite a substantial surplus of domestic natural gas (Davis 1984; Murray 1983; Uslaner 1989, chap. 5); and this fact was widely publicized through the efforts of the Citizen/Labor Energy Coalition (Pressman 1983). The *Washington Post*, in turn, gave Commerce Committee deliberations front-page coverage, and the issue was a high priority for the Reagan administration. The job training bill, likewise, was one of the most important domestic initiatives of Reagan's first term and received considerable media attention. The principal purpose of the bill was to replace the much maligned but widely used public jobs program, the Comprehensive Employment and Training Act (CETA), at a time when the national unemployment rate threatened to exceed 10% for the first time in four decades. To a lesser degree, finally, the 1982 dairy bill was also salient among actors off the committee and outside the industry. While the interest of the general public in dairy policy was slight, the burgeoning budget deficit loomed large on Capitol Hill, and it clearly motivated the decision to change dairy policy only one year after passage of an omnibus farm bill (Wehr 1982a, 1982b). Indeed, relative to other domestic nonentitlements, dairy subsidies were widely perceived as a major budget offender. The administration thus counted the price adjustments a high priority, one that commanded considerable attention from Budget Director David Stockman, and the House Budget Committee was involved at every stage of the process.

Finally, each of the policy areas we examine here has received the attention of previous scholars studying PAC contributions and floor roll calls; and in each case the effects of PAC money were found to be slight. In a study of dairy legislation considered in the House in 1975, for instance, Welch (1982) concluded that dairy PAC contributions were the least important determinant of voting on milk price supports and that their effect on the legislation was negligible (see also Chappell 1982). Grenzke (1989a) estimated a dynamic model of members' voting behavior over four congresses and found that labor union contributions had either a negligible or a *negative* effect on members' propensity to take prolabor positions on the House floor (but see Wilhite and Theilmann 1987). And Wayman and Kutler (1985) found no effect of natural gas industry campaign contributions on members' votes during House consideration of natural gas deregulation in 1975.

At two levels, then, past research indicates that our selection of cases is biased against our argument. It suggests that high salience issues should exhibit little PAC influence on legislative behavior, yet each of the cases here commanded the attention of a wide range of political actors. Second, past research suggests that we will find little PAC influence in precisely these three policy areas. Should we find support for the hypothesis that money mobilizes support (or demobilizes opposition) at the committee level, we should be on reasonably solid ground to conclude that (1) the results of this exploration are apt to generalize to other committees and other issues and (2) the null results of past research are more likely to be artifacts of the legislative behavior and the legislative stage studied than evidence that moneyed interests do not matter in congressional decision making.

The Model

The model of participation we use to test for the hypothesized effects is adapted from Hall 1987.[3] The model begins from the same motivational premise that we employed in our discussion of PAC contribution behavior. Members of Congress are purposive actors who allocate their time, staff, and other legislative resources in such a way as to advance certain personal goals or interests. There are several goals that commonly figure in these calculations. The one most prominently cited in the literature on legislative behavior is reelection or, more generally, service to the district (see, esp., Mayhew 1974); but we report elsewhere that the relevance of any particular goal to a member's participation depends directly on the nature of the issue and the legislative context (Hall 1987). To use language borrowed from Kingdon (1981), goals are "evoked." Any particular issue may evoke several goals simultaneously or may evoke none at all. In the latter case, a member is simply uninterested, the expected benefits of participation slight; in the former, the level of interest is intense, the expected benefits of participation high.

In the three cases under study here, in fact, several goals were probably at work in the resource allocation decisions of most committee members. For instance, the natural gas bill raised issues of government intervention in the economy and the country's long-term dependence on foreign energy sources. The budgetary implications of the dairy bill undoubtedly evoked some committee members' concerns about good fiscal policy and its macroeconomic consequences. The Job Training Partnership Act concerned the government's obligation to redress inequalities of economic opportunity resulting from inadequate or outdated job skills. But the goal most consistently evident in staff interviews, markup debates, and secondary accounts of the three bills was promoting or protecting district interests. For the purposes of this analysis, then, we adopt the simpler and more tractable motivational assumption common to most models of legislative behavior.[4] In deciding whether and to what extent to participate on a particular issue, the member estimates both the expected benefits and expected costs, where benefits are a direct function of the issue's economic relevance to the district.[5]

If the interests of one's constituents motivate a member to become involved, the costs of participation are also important and highly variable: resources are scarce, and the allocation to one activity results in other profitable opportunities foregone. Several factors affect the resources available to particular members on particular issues. First, assignment to the subcommittee of jurisdiction provides members both with greater formal opportunities to participate and access to an earlier stage of the sequential process. It also gives the member greater access to staff and to lines of communication with other interested actors both on and off the committee. For similar reasons, a committee or subcommittee leadership position subsidizes participation even more. The greater staff allocations that these positions bestow, the procedural control over the agenda, and the central place in the committee communication network diminish the time and information costs associated with meaningful involvement in the issue at hand. Finally, freshman status tends to increase the information costs and diminish the opportunities or resources a member enjoys for any particular bill.

The variable of greatest interest in this investigation, however, is the level of contributions each member receives from PACs interested in the issue at hand. To what degree, that is, does money affect members' decisions regarding whether and to what extent they will participate in the committee deliberations? Two points require emphasis here. First, the foregoing discussion suggests that the effects of money on participation should not be simply linear. The positive effect of contributions on participation should be contingent on probable support; this is the mobilization hypothesis. To the extent that contributions are given to probable opponents, on the other hand, they should diminish participation; this is the demobilization hypothesis.

Second, contributions may well be related to other activities that moneyed interests employ to further their legislative aims, making it difficult to isolate the effects of any particular part of their effort (Rothenberg 1989; Wright 1989). For instance, it may be the case that those groups that organize PACs for the purpose of channeling money to candidates are also the most active in developing grass roots campaigns or direct lobbying efforts. While there is evidence to suggest that the correlation among these activities is modest for the cases under study here,[6] our data on interest group activity are limited to political action committee campaign contributions. Hence, while our model tests for the effect that money has on committee behavior, one might more accurately characterize our results as capturing the effect of the several resources that moneyed interests employ.[7]

The dependent variable is the participation of member *i* on bill *j*, where participation refers to a member's activity both during formal committee markups and committee action behind the scenes. Our data on activity are drawn from two sources: semistructured interviews with both the majority and minority staffers assigned to cover each bill and the largely unpublished but meticulously kept committee and subcommittee markup records. The summary measure of participation that we use for the purposes of this exploration is a simple scale score derived from a factor analysis of six activities: attendance; voting participation; speaking; offering amendments during committee markups; role in authoring the legislative vehicle or an amendment in the nature of a substitute; and negotiating behind the scenes at either the member or the staff level.[8] The measurement of the independent variables, in turn, follows directly from the preceding discussion. Members' institutional positions and status are measured with dichotomous variables that are set at zero except as the following conditions hold: subcommittee membership takes a value of one if a member sat on the subcommittee with jurisdiction over the bill; leadership position takes a value of one if a member was chair or ranking minority member of either the full or subcommittee; and freshman status takes a value of one for members in their first term in the House.

In measuring the relevance of each issue to committee members' districts, we assume that relevance is primarily economic in nature. In the natural gas case, this takes two quite different forms: total district-level natural gas production[9] and the economic effect of gas price increases on residential consumers in the member's district, which we measure using industry data on natural gas price increases and census data regarding congressional district natural gas use.[10] If high production and high inflation capture dimensions of intradistrict salience, however, the presence of both at once should produce intradistrict conflict. The member is torn between two significant economic interests, and activity on behalf of one may alienate the other. Indeed, Fiorina (1974) suggests that unrequited constituents are likely to punish more than the requited are to reward. As intradistrict conflict increases, in any case, the expected benefits of activity on the issue should diminish, ceteris paribus. In the natural gas case, then, intradistrict conflict occurs as the production and inflation variables both approach their upper limits. We measure this condition as the product of two terms: "high production" is the extent to which natural gas production in the district exceeds the mean district production for all members of the committee; similarly, "high inflation" is the extent to which the district inflationary effect exceeds the mean for all committee members. When either district gas production or inflationary effect is below the committee mean, then, intradistrict conflict is zero.

In the other two cases the measurement of district interest is uncomplicated by potential conflicts within members' geographic constituencies. In the dairy stabilization case district relevance is directly related to the importance of dairy farming, measured simply by the total number of dairy cows in the member's district as reported by the United States Department of Agriculture biennial census. Given that milk prices were not a salient consumer issue per se and that the Dairy Stabilization Act was not likely to affect retail prices in any significant way, we do not assume a more general public concern with this issue. For the Job Training Partnership Act, likewise, district relevance is directly related to the importance of federal jobs programs in addressing structural unemployment, which we measure as the current level of CETA expenditures in the member's district.[11] This variable not only taps the district-specific economic benefits of clients of the expiring job training program but (given that CETA allocations were directly tied to local unemployment rates) also captures the severity of structural unemployment in the district.

Consistent with the preceding theoretical discussion, we estimate the effect of group expenditures on participation by including pairs of interactions between group contributions (measured as the amount contributed during the two-year election cycle prior to committee action) and indicators of probable support or opposition. For each case, the exact specification of the interactions is straightforward. In the dairy stabilization case, we measure probable support or opposition using the ratings of the National Farmers' Union (NFU),[12] an organization that strongly supports federal intervention in the agricultural economy to control supply and support the commodity prices paid to farmers. Given that we expect very different effects for contributions on the behavior of likely supporters and opponents, however, the model requires two separate interactions: *Money to supporters* is the product of contributions[13] and the member's distance from the mean NFU score where the members' rating is greater than the mean; the money-support term is zero otherwise. *Money to opponents* is the product of contributions and the member's distance from the mean NFU score, where the member's rating is less than the mean; the money-opposition term is zero otherwise. Following the theoretical reasoning of the last section, then, the expected effect on participation is positive for money to supporters. The expected effect is negative for money to opponents in each case.

Any attempt to model the effect of contribution activity on legislative behavior cannot assume that a particular industry is necessarily unified, however: one segment of an industry may have different interests and work in ways that offset some other segment. In the case of the federal dairy legislation, no such split within the industry was apparent among the principal actors, thus permitting the fairly simple specification described above. But in general—and in the natural gas case in particular—an industry may not be so easily simplified. While the gas producers were by far the most visible and most vigorous among the corporate actors and gave by far the most money in campaign contributions among energy PACs, the natural gas industry was seriously divided (Pressman 1983; Uslaner 1989, chap. 5), a feature that we attempt to capture. The alignments were by no means perfect, but the principal issues at stake in the legislation before House Energy and Commerce pitted the major gas producers and intrastate pipelines against the interstate pipelines and distributors. As a result, different segments of the industry were likely to target different members to serve as legislative agents and identify different members as their likely opponents. Our first task therefore was to distinguish the various energy PACs according to the principal business activities of their affiliates. Using the detailed descriptions of individual companies provided by Moody's Investor Service (1983a and 1983b), we classified each affiliate according to its principal interests in the natural gas area.[14] We then divided the contributions a member received according to whether they came from producers or intrastate pipelines on the one hand and interstates or distributors on the other. The measure of contributions that we employ, then, is the producer-intrastate contributions minus the interstate-distributor contributions, the value of which was positive in almost every case.

The operationalization of the interactions tapping the net producer-intrastate effects, in turn, was handled in a fashion analogous to the dairy stabilization case. In the natural gas case, however, members' Americans for Democratic Action (ADA) scores were more appropriate as an indicator of likely support or opposition. For the producer-intrastate segment of the gas industry at least, the issue of greatest concern was the extent to which the government continued its intervention in the natural gas market by controlling the price of old gas. The ADA score should tap members' historical tendency to support such federal interventions quite well. *Money to supporters,* then, is the product of net producer-intrastate contributions and the member's distance from the mean ADA score where the member's rating is less than the mean; the money-support term is zero otherwise; and *money to opponents* is the product of contributions and the member's distance from the mean ADA score where the member's rating is greater than the mean; the money-opposition term is zero otherwise.

Unlike the dairy and natural gas cases, finally, the job training bill did not involve issues specific to a particular industry. The organized interests most concerned with CETA and its prospective replacement were the national labor unions: public service employment and training programs were at the top of labor's agenda, especially in 1982, when unemployment was approaching postwar records. Moreover, labor unions were one of the single largest categories of contributors to congressional campaigns and gave to five-sixths of the members of House Education and Labor. It is the effect of these contributions on committee behavior with which we are primarily concerned. This is not to say, however, that labor unions were the only groups interested in mobilizing support on this bill.[15] On the business side, national business associations generally opposed any public service employment provisions and favored an expanded role for private industry councils so that federally subsidized training would be tailored to meet the changing needs of the private sector (Baumer and Van Horn 1985, 173). As in the natural gas case, we thus employ a net contributions variable, which takes the value of the member's total labor contributions less the total contributions received from national business organizations.[16] As in the other two cases, likewise, the indicator of probable support or opposition was constructed using the appropriate group rating, in this case, the AFL-CIO's Committee on Political Education (COPE) score. *Money to supporters,* then, is the product of net labor contributions and the member's distance from the mean COPE score, where the member's rating is greater than the mean; and *money to opponents* is the product of contributions and the member's distance from the mean COPE score where the member's rating is less than the mean.

Results and Interpretations

In estimating the model of participation, we explicitly account for the possibility that contributions are effectively endogenous, that is, that in allocating contributions to committee members during the previous election cycle, a group may attempt to anticipate who the principal players will be on issues it cares about.[17] To the extent this is true, at least, the error term will be correlated with contributions and the ordinary least squares coefficient on the latter will be upwardly biased. We thus estimate the participation model using two-stage least squares, with the second stage results reported in the tables.[18] In each of the three cases, the model performs quite well, explaining over 55% of the variance in participation. More importantly, the analysis provides solid support for the principal hypothesis of this study, that moneyed interests mobilize bias in committee decision making.

This finding is clear for all three cases.[19] The campaign contributions that dairy industry PACs gave to their likely supporters significantly increased their participation, even when we controlled for the importance of the issue to individual members' districts, whether they sat on the subcommittee of jurisdiction, and whether they held a leadership position (Table 1). Such factors are reported elsewhere to be strong determinants of committee participation (Hall 1987), and each is also likely to affect contributions since interest groups tend to concentrate their resources on members who hold positions of institutional power (e.g., Grenzke 1989b; Grier and Munger 1986, 1989), as well as on members who have a district stake in their industry. That the mobilization coefficient remains positive and significant in the face of the multivariate controls reinforces the interpretation that the connection between group resources and mobilization is causal. When dairy PACs did give to their probable opponents, moreover, there is some evidence that the contributions diminished participation. While the coefficient on the money-opposition variable is statistically insignificant, its size is substantively nontrivial, and the negative sign is consistent with the demobilization hypothesis. In short, the more money a supporter received from the dairy PACs and the stronger the member's support, the more likely he or she was to allocate time and effort on the industry's behalf (e.g., work behind the scenes, speak on the group's behalf, attach amendments to the committee vehicle, as well as show up and vote at committee markups). Alternatively, money may have diminished the intensity of the opposition. The effect of money on decision making in the House Agriculture Committee, then, was to encourage industry supporters to be active and, if anything, to encourage industry opponents to abdicate.

The results of the job training case are also clear, and the specific estimates are striking in their similarity to the dairy stabilization case. As Table 2 shows, the contributions that labor groups made to their supporters had a substantial, statistically significant effect on participation during Education and Labor deliberations. Remarkably, the unstandardized coefficient for the money support variable is almost identical in size to the analogous coefficient in the dairy stabilization model

Table 1. PAC Money and Committee Participation: 1982 Dairy Stabilization Act

Independent Variables	Unstandardized 2SLS Coefficient	t-statistic
Intercept	.01	.05
Number of dairy cows in district	.27**	2.21
Dairy PAC contributions to supporters	.26**	2.42
Dairy PAC contributions to opponents	−.11	−.61
Membership on reporting subcommittee	.17**	3.54
Committee or subcommittee leadership position	.35**	4.50
Freshman status	−.02	−.31

Note: Adjusted R-squared = .60; number of observations = 41. All variables are measured on a 0–1 scale. The contributions term is the predicted value from the first-stage equation.

**Statistically significant at .05 level, one-tailed test.

Table 2. PAC Money and Committee Participation: 1982 Job Training Partnership Act

Independent Variables	Unstandardized 2SLS Coefficient	t-statistic
Intercept	.13	.77
CETA expenditures in district	.03	.23
Labor union net contributions to supporters	.25*	1.62
Labor union net contributions to opponents	−.18	−.80
Membership on reporting subcommittee	.19**	2.61
Committee or subcommittee leadership position	.47**	4.55
Freshman status	−.05	−.51

Note: Adjusted R-squared = .56; number of observations = 32. All variables are measured on a 0–1 scale. The net contributions term is the predicted value from the first-stage equation.

*Statistically significant at .10 level, one-tailed test.
**Statistically significant at .05 level, one-tailed test.

despite the fact that the two cases are drawn from different committees with qualitatively different jurisdictions and policy environments (Smith and Deering 1984). In each case, a change in the money support variable from its minimum to its maximum value moves a member approximately one-fourth of the way along the participation scale, almost exactly one standard deviation. In both cases, likewise, this coefficient is greater than that for subcommittee membership, a variable generally considered central to understanding participation in the postreform House. As in the dairy stabilization case, finally, the Education and Labor bill provides some support for the demobilization hypothesis. While it fails to meet conventional levels of statistical significance, the size of the money-opposition term proves negative and substantively significant, nearly matching the size of subcommittee membership.

The results regarding moneyed interests and mobilization are only slightly less compelling in the natural gas case, a case complicated both by divisions within the industry and the apparent importance of both organized and unorganized interests. As we note above, such conditions are likely to mitigate the efficacy of interest group efforts, and they complicate the measurement of anticipated support and opposition. Still, the mobilization hypothesis finds strong support in the behavior of Energy and Commerce members. While the size of the unstandardized coefficient for the money support variable is somewhat smaller than for the other two cases, it is still substantial and statistically significant at the .05 level. A change in the money support variable from its minimum to its maximum moves a Commerce Committee member approximately one-sixth of the way along the participation scale. By way of illustration, this amounts to the difference between Minnesota Representative Gerry Sikorski, who did little more than faithfully attend and vote during formal markups, and Alabama Representative Richard Shelby, whose staff participated in behind-the-scenes negotiations and who offered two substantive amendments during subcommittee markup, both of which passed.

As Table 3 shows, finally, the demobilization hypothesis is not supported in the natural gas case. While the coefficient on the money opponents interaction is slight, its positive sign is inconsistent with our

Table 3. PAC Money and Committee Participation: 1984 Natural Gas Market Policy Act

Independent Variables	Unstandardized 2SLS Coefficient	t-statistic
Intercept	.08	.40
Natural gas production in district	.32*	1.65
Natural gas price increase effect on district	.17*	1.35
High production/high inflation interaction	−.18	−1.28
Producer-intrastate net contributions to supporters	.17**	1.69
Producer-intrastate net contributions to opponents	.01	.06
Membership on reporting subcommittee	.23**	3.17
Committee or subcommittee leadership position	.54**	4.77
Freshman status	.13*	1.31

Note: Adjusted R-squared = .57; number of observations = 42. All variables are measured on a 0–1 scale. The net contributions term is the predicted value from the first stage equation.

*Statistically significant at .10 level, one-tailed test.
**Statistically significant at .05 level, one-tailed test.

prediction. The foundation for the demobilization hypothesis being theoretically weaker, however, the null result here, as well as the weak results in the dairy and job training cases, are not altogether surprising. The theoretically stronger hypothesis, that money mobilizes a pro-PAC bias at the committee level, is confirmed in all three.

For the most part, the other variables in the model also perform as predicted and suggest interesting implications for the politics of representation in a decentralized Congress. The relevance of an issue to the member's district enhances member participation in two cases, providing evidence that Agriculture and Commerce members purposively allocate their legislative time and resources to promote the interests of their constituencies. On House Agriculture, the more important dairy farming was to the member's district, the more likely he or she was to participate in committee deliberations. Likewise, the greater the presence of natural gas production in the district, the more likely the Energy and Commerce member was to participate in deliberations on the Natural Gas Market Policy Act. Indeed, a change in gas production from its minimum to its maximum corresponds to a 32% change along the participation scale, the difference between simply showing up and being a major player on the bill. By comparison, however, the effect of natural gas price increases on district consumers appears smaller by half. And the importance of structural unemployment and program spending in the districts of Education and Labor members had at best a slight effect on their involvement in the Job Training Partnership Act.

Pending better measurement of unorganized constituents' interest at the district level, of course, we cannot draw unqualified conclusions regarding their importance in shaping committee behavior. Should such patterns hold up under subsequent analysis, however, the implications for member responsiveness to industry interests and industry money relative to more general constituency concerns would be several and important. If members allocate their scarce legislative time and resources with district interests in mind, they perceive their districts in terms of different constituencies; and these perceptions affect their behavior as representatives (Fenno 1978). In part, the results presented here suggest that organized economic interests within districts figure more prominently in the psychology of representation than the diffuse and unorganized interests of rank-and-file voters. Such was the charge that Schattschneider made thirty years ago, one which critics of pluralism have echoed repeatedly since.

At the same time, however, the findings in the natural gas case also suggest that the preferences of unorganized interests sometimes constrain the responsiveness of members to organized groups, thus confirming the thesis of Denzau and Munger (1986) regarding how unorganized interests get represented. Beyond the positive coefficient for the inflationary effect variable, this is evident in the size and significance of the coefficient on the high production–high inflation interaction. Even if members are inclined to respond to producer interests, in short, this tendency is mitigated when consumer interests are also high. However, we should point out two things. First, the simultaneous occurrence of both strong producer interests and high consumer-voter salience is rare. Indeed, this distinguishes the natural gas issue from most of the issues with which members of Congress typically deal, and even in this case only 4 of the 42 members of Energy and Commerce were seriously cross-pressured. Second, we found no such constraint on the behavioral effect of producer contributions. One might expect, for instance, that the mobilizing effect of producer contributions would be diminished for a member who also represents a high inflation district. In one variant of the model tested here we included an interaction between the money support and high inflation variables, with the result that the coefficient was correct in sign (negative) but very near zero and the money-support coefficient was unchanged.

Finally, most of the variables that tap members' institutional positions prove to be strong determinants of committee participation. While the coefficients on freshman status differ in sign, both subcommittee membership and leadership position are positive, statistically significant, and substantively large in all three cases. Even on issues that are widely perceived among the committee membership to be important, issues where the organized interests in the policy environment are themselves active, the opportunities and resources provided by formal institutional position are major factors in determining who makes the laws at the committee stage. Such findings are generally consistent with findings from other committees and larger samples of issues (Evans n.d.; Hall 1987, 1989; Hall and Evans 1990) and reinforce the assumption that the model of participation employed here is specified correctly.

Conclusion

We have elaborated a theory of the member-group exchange relationship that comprehends the general patterns of PAC contributions reported in the literature. House members and interest group representatives are parties to an implicit cooperative agreement, but the constraints on member behavior and the rational calculations of group strategists limit the extent to which votes become the basis for exchange. This view suggests expectations about the effects of money on congressional decision making quite different from the ones that motivate the substantial research on the subject. We should find little causal connection between contributions and votes, especially on the floor—an expectation generally supported, although not adequately explained, in the literature. We should expect to find an important connection between contributions and the legislative involvement of sympathetic members, especially in committee—a relationship that empirical research to date has altogether ignored.

In order to test this view of moneyed interests and congressional decision making, we investigated the participation of House members on three issues in three committees. In each case, we found solid support for our principal hypothesis: moneyed interests are able to mobilize legislators already predisposed to support the group's position. Conversely, money that a group contributes to its likely opponents has either a negligible or negative effect on their participation. While previous research on these same issues provided little evidence that PAC money purchased members' votes, it apparently did buy the marginal time, energy, and legislative resources that committee participation requires. Moreover, we found evidence that (organized) producer interests figured more prominently than (unorganized) consumer interests in the participation decisions of House committee members—both for a case in which the issue at stake evoked high district salience and one where it did not. And we found little evidence that committee members respond to the interests of unemployed workers except insofar as those interests might be represented in the activities of well-financed and well-organized labor unions. Such findings suggest several implications for our understanding of political money, interest groups, and the legislative process.

The first and most important implication is that moneyed interests *do* affect the decision-making processes of Congress, an implication that one does not easily derive from the existing political science literature on contributions. In fact, it matters most at that stage of the legislative process that matters most and for a form of legislative behavior likely to have a direct bearing on outcomes. As David Mayhew has suggested (1974, 95), parliamentary suffrage gives a member relatively little leverage over the shape of legislation, especially at the committee stage. Only a small fraction of the decisions that shape a bill ever go to a vote, either in committee or on the floor. The vast majority are made in authoring a legislative vehicle, formulating amendments, negotiating specific provisions or report language behind the scenes, developing legislative strategy, and in other activities that require substantial time, information, and energy on the part of member and staff. While such efforts by no means guarantee that a particular member will influence the final outcome, they are usually a precondition for such influence (Hall 1989).

A second and related implication of this investigation, then, is that empirical research should expand its view of the legislative purposes of political money and the other group resources that may accompany it (see also Salisbury 1984, esp. 70–72). We focus here on committee participation; but the more general implication is that group expenditures may do much more than buy votes, or they may buy votes under certain conditions and affect other forms of legislative behavior under others. Such a suggestion, of course, usually appears in the various studies that examine the relationship between contributions and floor roll calls, but it needs to be elevated from the status of footnote or parenthetic remark to a central element of future research designs. Even for a small set of issues and a single group, the legislative strategies available are several, sometimes mixed. To speculate beyond the research reported here, for instance, we believe groups allocate their various resources (1) to mobilize strong supporters not only in House committees but also on the Senate floor, in dealings with executive agencies, and in various other decision-making forums relevant to the group's interests; (2) to demobilize strong opponents; and (3) to effect the support of swing legislators. We require greater knowledge of the frequency and efficacy of such strategies, in any case, before we denigrate the role of moneyed interests in Congress, especially when the overwhelming weight of the evidence provided by Washington journalists and political insiders suggests that they matter a great deal.

Finally, the argument presented here provides a very different slant on the role of interest groups as purveyors of information in the deliberations of representative assemblies. A common defense of group lobbying activity, in fact, is that it provides ideas and information although its effect on member preferences is slight. Members (and their staffs) tend to consume information selectively, relying on sources with whom they already agree and discounting sources with whom they usually disagree (e.g., Milbrath 1963). The view that we have advanced here suggests that while this may in fact describe how such information is used, it does not render it inconsequential. In light of the extraordinary demands on each congressional office, information—gathering it; analyzing it; turning it into speeches, amendments, and bills; using it to develop legislative strategy—can be very costly. Such costs, more than anything, limit the extent to which a nominal member will be a meaningful player in the decision-making process on a particular bill. At the very least, then, money-induced activity will distort the "representativeness of deliberations," a standard that democratic theorists since John Stuart Mill have used to evaluate the legitimacy of legislative assemblies (Chamberlin and Courant 1983). But it may also affect the "representativeness of decisions." By selectively subsidizing the information costs associated with participation, groups affect the *intensity* with which their positions are promoted by their legislative agents. In short, not all preferences weigh equally in legislative deliberations; and the resources of moneyed interests at least partly determine the weights.

The extent to which such efforts are damaging to representative government, as Schattschneider claimed, depends in part on the balance of interests and resources apparent in the relevant set of groups that are organized for political action. On any given issue, the efforts of one interest to mobilize supporters in Congress may be at least partially offset by the efforts of some competing group to mobilize its own supporters; indeed, there is some evidence that such countervailing efforts occurred in the natural gas case. But for those who believe that money is an illegitimate resource in such efforts—that pluralism requires something more than a competition among moneyed interests—the results of this study can only be disturbing.

Notes

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1. Rothenberg (1989) finds that in the allocation of lobbying resources on the MX missile issue Common Cause did concentrate more on likely "fence straddlers." By extension, his analysis provides an excellent guide for modeling the effect of expected voting behavior on contributions. See Smith 1984 for an important formulation of this argument.

2. Kingdon found that there was no conflict in the member's "field of forces" in almost half of the important votes that members cast on the House floor. In an additional 33% of the votes all of the personal goals that were relevant to a vote pointed the member in the same direction (1981, 255). While his study was conducted before the precipitous rise of

PAC contributions, Kingdon found little evidence of group influence on members' voting decisions (chap. 5).

3. In adapting our model from Hall 1987, we retained only the variables that were found to be consistently significant and collapsed the several specific leadership positions into a single dichotomous variable.

4. This assumption is especially valid for the dairy and natural gas cases, though somewhat problematic for the JPTA. Like so many of the issues that come before the House Agriculture Committee, the dairy program is a classic constituency issue. If anything, the administration's assault on the price levels intensified such interests in the minds of the legislators. Likewise, the Natural Gas Market Policy Act evoked strong sentiment among consumers, distributors, and pipelines in some states and producers in others, sentiments that were loudly communicated to their representatives in Washington (Murray 1983). The resulting regional split within the committee was noted at length in virtually every account of its deliberations (see e.g , Maraniss 1983; Murray 1983).

5. In attempting to capture the representation of constituency interests, however, we necessarily neglect constituents' preferences regarding the public goods dimensions of each of these bills. On the importance of public goods preferences to political representation, see J. Jackson and King (1989).

6. The principal grass roots campaigns in the natural gas case were conducted by the Citizen/Labor Energy Coalition (CLEC) (which conducted door-to-door efforts in a number of states) and the public utility companies (who used inserts in monthly utility bills to encourage their customers to write letters to their representatives). Both were also actively engaged in lobbying members of the Energy and Commerce Committee. (Indeed, the CLEC was one of the most vigorous in this respect; see Pressman 1983.) Neither of the two were major campaign contributors, however. The CLEC did not have an organized PAC, and of the various segments of the gas industry the utility companies contributed relatively little money. (The major gas producers, for instance, contributed more than the distributors by a factor greater than seven to one.) Similarly, there were dozens of groups active in lobbying on the Job Training Partnership Act that contributed little or nothing in the way of campaign money, including various public interest groups, state and local officials, education organizations, and the National Governors' Association (Baumer and Van Horn 1985). The correlation between contributions and other interest group activities is probably higher for the dairy stabilization case, but even here it should be fairly modest. The various dairy organizations were in fact active in getting local dairy producers to write letters and meet with their representatives during visits to the district. But such a grass roots strategy was only feasible in districts that had a significant number of dairy producers, and the correlation between district dairy production and dairy industry PAC contributions was only .09 for the period 1979–80. Likewise, while the dairy industry gave a great deal of money to some House Agriculture members and none to others, the National Milk Producers by themselves contacted *every* committee member regarding the dairy stabilization bill, either through letters to the member's Washington office or meetings with the member or the member's staff.

7. On this point, we are especially indebted to conversations with Jack Wright.

8. Data on the first four activities were taken directly from the committee and subcommittee markup minutes and transcripts. Indexes of authorship role and behind-the-scenes participation were coded on four-point scales from semistructured interviews with both minority and majority staffers who had primary responsibility for staffing the bill under study. On the collection and coding of these data, see Hall 1987, though the data-reduction technique used here loses less information than the Guttman scale scores and the informal participation indexes that were used in that analysis. The factor analysis that generated the scores retained only one factor using conventional methods, the weights assigned were similar across the three cases, and (most importantly) the ordinal ranking of the weights for each activity were precisely those hypothesized in Hall 1987. In addition to the results reported here, however, we also estimated the model using both the Guttman scales and the informal participation index as well as a simple summary of the two. These several measures of participation are all highly correlated, and various estimates of the model using them generally confirmed the findings that we report here. Problems of measurement undoubtedly remain, however; and addressing them is an important matter for future work.

9. The measure of district natural gas production was constructed from county-level data acquired directly from state departments of natural resources. Where counties were not wholly contained within a single district, the proportion of natural gas production credited to particular districts was estimated by comparing congressional district maps with the geologic surveys showing the geographic location of natural gas production within counties. The production data are for the year 1983, the year in which the Energy and Commerce Committee began consideration of the Natural Gas Market Policy Act.

10. District-level data on natural gas price changes were not immediately available, but the intrastate variations should be sufficiently small as to make the state-level data reasonable approximations of the inflation in district natural gas prices. However, there are dramatic variations in the use of natural gas from one district to the next, so that the economic effect of a given price increase on residential energy

consumers may vary dramatically across disticts within a state (e.g., many rural districts that depended primarily on fuel oil for home heating were virtually unaffected by major increases in the price of natural gas). Fortunately, however, district-level information about household fuel use is available. In order to create a district-level indicator of consumer interest, then, we simply multiplied the state-level price increase for 1981–82 times the percentage of households in the district that used natural gas for their home heating. State data on the average price of natural gas delivered to consumers were taken from the *Natural Gas Annual 1982* (vol. 1, Table 17) and the *Natural Gas Annual 1983* (vol. 1, Table 18). District-level data on household energy sources were taken from the U.S. Bureau of the Census 1981.

11. District-level data on CETA expenditures were calculated from *The Employment and Training Reporter* (1980), which lists the 1981 allocations to counties, cities, or other "prime sponsors" located within members' districts. In cases where a prime sponsor was located in more than one congressional district, the expenditure for that sponsor was allocated equally among the several districts in which the sponsor administered its program.

12. It is important to note, however, that we are not assuming that NFU, ADA, or any other voting index measures members' personal ideology (much less their true preferences); a number of factors combine to determine these voting patterns, ideology being only one. (See Jackson and Kingdon 1990; Carson and Oppenheimer 1984.) Rather, we simply assume that the rating summarizes members' past voting behavior, which in turn form the basis for particular groups' expectations about what positions members will take in the future. Indeed, one of the principal reasons that groups construct their own indexes is to help them distinguish between friend and foe, and raters themselves report that the ratings "have their greatest impact on the distribution of campaign funds, because they provide a simple test of support or opposition" (Fowler 1982). The NFU scores were taken from the National Farmers Union Newsletter (1982a, 1982b).

13. We measured dairy industry contributions for each member as the summary of contributions from the three main dairy PACs during the previous election cycle: Committee for Thorough Agricultural Political Education of Associated Milk Producers; Mid-America Dairymen; and Dairymen Special Political Agricultural Community Education.

14. The Moody's entry included a brief description of each business's activities that usually indicated whether it belonged primarily in one category or another. Where that description mentioned interests in more than one category, we went to the financial statements or audit summaries provided in the Moody's entry and classified businesses as producer, interstate pipeline, intrastate pipeline, or distributor according to the principal sources of their natural gas revenues. Such information permitted an unambiguous classification in almost every case. Natural gas peak associations were categorized according to the nature of the businesses they represented. In addition, some of the classifications were checked against similar classifications made by Eric Uslaner using both interview and archival data. In every case where our data overlapped, our classifications matched his (see Uslaner 1989).

15. Other groups interested in the legislation included the National Governors' Association, national education groups, city and county officials, and the various organizations that represented them, such as the National League of Cities (Baumer and Van Horn, 1985). Of these, however, only the education groups contributed money; and they tended to align with, and contribute to, the same members as organized labor. The education contributions were very small in any case, and the alternative strategy of adding them to the labor PAC total had no effect on the coefficients.

16. Included in this category were the American Business Association, the Business Industry Council, the Chamber of Commerce, the National Association of Manufacturers, and the National Federation of Independent Businesses.

17. We believe that there is far less reason a priori to believe that PAC contributions should be considered endogenous in modeling members' participation than in modeling their roll-call voting behavior. While it is likely that PACs will give disproportionately to members with important committee positions, there is little evidence to suggest that the anticipated participation of member *i* on some particular bill *j* (independent of what one would anticipate given the member's institutional position or positions, seniority, and interests—factors that are built into our model) figures prominently in PAC allocation decisions. Such calculations, at least, have been nowhere evident in the considerable political science or journalistic literature on this subject. Hence, we also estimated the equations for both cases using ordinary least squares. The parameter estimates from the ordinary least squares and two-stage least squares (2SLS) were very similar, with the exception that the magnitude of the 2SLS mobilization coefficients were somewhat smaller in the natural gas and job training cases. By presenting the 2SLS results, then, we address the potential endogeneity problem and, as it turns out, slightly bias our results against our main conclusions.

18. While the first-stage results are not relevant to our substantive interests here, they do bear on the confidence of the second-stage results and thus warrant some attention. In estimating the first stage, we adapted the contributions model from the substantial literature on the allocation strategies of national PACs (e.g., Evans 1986; Grenzke 1989b; Gopoian 1984; Grier and Munger 1989), including three variables that qualified as instruments: party, the rele-

vant voting index, and the marginality of the district. In all three cases the first-stage results were satisfactory. The adjusted R-squared was .34 for the model of dairy industry contributions to Agriculture Committee members, .58 for the model of net producer contributions to Commerce members, and .48 for the model of net labor contributions to Education and Labor members. More importantly, in every case the coefficient on at least one of the three instruments was large, correct in sign, and statistically significant at the .05 level. Checks for multicollinearity among the independent variables in the second stage equations likewise provided little cause for concern. Regarding the appropriateness and implementation of the two-stage least squares estimation procedure, see Hanushek and Jackson 1977, chap. 9; Pindyck and Rubinfeld 1981, 328–31.

19. As a check on the results reported here, we also estimated for the effects of contributions on participation without interacting them with anticipated support or opposition, and, as our theory would predict, the effects were consistently weaker. Note, secondly, that we do not include the relevant group support score separately in the model, a variable that has proven important in estimating the effects of contributions on roll calls. Even if ideology is what the voting scores capture (see Jackson and Kingdon 1990), there is no theoretical reason to expect that liberals will be more active than conservatives (or vice versa) or for that matter that ideological moderates will be less active than either conservatives or liberals. In any case, we tested for the effect of past voting behavior on the participation of members in each case. For all three, the t-statistics for the voting score coefficients were less than .5.

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SOVIET CITIZEN PARTICIPATION ON THE EVE OF DEMOCRATIZATION

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We reassess the debate over Soviet citizen politics in the USSR during the Brezhnev era. We argue the need for a more complex model of citizen participation in the USSR before Gorbachev if we are to have an accurate baseline for evaluating changes in regime-society relations. We examine the connections between individual attitudes and individual behavior and show that political participation under the "old regime" was not nearly as one-dimensional and devoid of effect as many previous researchers (and current Soviet leaders) have described it. Many forms of political participation in the Soviet Union before Gorbachev did not fit the stereotype of a psychologically disengaged citizenry driven to participate only by coercion, a desire to conform, or a quest for particularized benefits from public officials.

One of the most visible products of reform in the Soviet Union under Gorbachev has been the transformation of mass politics. The rise of thousands of informal groups and the defeat of many candidates supported by the Communist party in the March 1989 election marked a revolutionary turn in political participation in the USSR. Soviet citizens displayed a degree of sophistication in their nominating and vote-garnering strategies, as well as in casting their ballots, that few students of Soviet politics would have predicted. The subsequent development of parliamentary institutions and the breaking of the Communist party's monopoly of power carried the political revolution even further.

The new politics has been accompanied by a reappraisal of political involvement in the era before the advent of Gorbachev's democratization. Analysts in the USSR now emphasize that the level of participation in that era fell far short of the mass activism prescribed in Soviet theory. And they question how authentic participation in fact was, to what extent it reflected real personal commitment, and to what extent it was simply a response to pressures for conformity.

The debate echoes a lively controversy among Western scholars over the nature of Soviet citizen participation under Khrushchev and Brezhnev (for overviews, see Hahn 1987 and Schulz 1981). While many analysts have described participation then as almost entirely a product of regime-directed *mobilization*, others have suggested that citizens' motives and opportunities for political activism could not be as narrow as the mobilization model implied. However, evidence to sort out these competing arguments is scant and contradictory. If one study suggests a positive connection between political interest, individual values, and political involvement, another finds apathy and alienation prevalent among those who were formally active in politics before democratization.

We reexamine the connection between individual attitudes and political behavior on the eve of the Gorbachev era. Although many Soviet and Western authors now dismiss citizen activism in the so-called period of stagnation, we shall present evidence that political involvement was hardly as one-dimensional and devoid of affect as it is currently described. We argue that there is a critical need for a more complex model of participation before Gorbachev if we are to have an accurate baseline for evaluating change in Soviet citizens' orientations toward the political world. The theoretical framework for analyzing political involvement needs to be recast in order to better explain the levels and types of citizen activism.

We also draw some insights from comparative theories of political participation. As analysts from Verba and Nie (1972) onward have emphasized, there may be little value in treating participation as a single, unidimensional construct. Different forms of political involvement offer a different mix of costs and benefits to potential activists, and these affect who will become active (see, esp. Barnes and Kaase 1979; Verba and Nie 1972; Verba, Nie, and Kim 1978).

The Nature of Soviet Political Participation

One of the appeals of the mobilization model was that it fit well with the Soviet tradition of political control. Not only were Soviet political institutions authoritarian, but society itself seemed inhospitable to democratic values. Analysts wondered whether Soviet political culture would support democratic political processes and open political debate; at best, it seemed to encourage subject competence and covert participation (Barghoorn and Remington 1986; DiFranceisco and Gitelman 1984).

Moreover, the mobilization model suggested that the Soviet citizen was largely depoliticized and disengaged psychologically from the political process. Sharlet (1967) contended, for example, that those who participated in public activities organized by the state were no more interested and no more committed to the system than the few who did not and that they were no more efficacious than those who did not. Roeder (1989a) suggested that *enforced departicipation* might be a more appropriate term to describe citizen involvement. And Shtromas (1984) argued that political action reflected not the internal acceptance of regime norms but merely an external sign of willingness to conform. Being a conventional political activist therefore said little about what a person believed.

DiFranceisco and Gitelman (1984, 605) took this notion of political disengagement a step further, contending that the Soviet citizen dismissed official channels of participation as a mere formality. There was "a meaningful form of participation," but it was "limited to affecting political outputs that concern the individual directly." Although some people might derive emotional rewards from taking part in conventional public organizations, the one activity in which people invested the most energy was contacting public officials to obtain individualized benefits, which DiFranceisco and Gitelman labeled "covert participation." Citizens were cynical about the formal institutions of conventional politics, preferring instead to work the output side of the system for personal gain.

Analysts skeptical of the mobilization model contended that individuals had more options and more varied ideas about their own roles in public life. As Little (1976) observed, the wide variety of public organizations in the USSR meant that citizens had some choice of which groups to join and how much to participate in any one of them (see also Baylis 1978; Schulz 1981). Citizens might also choose not to participate at all. And this, in turn, implied that individual values might play a role in determining how active each citizen would become. Hough (1976) contended that the notion of mobilization might apply to some forms of political involvement such as voting, but that other types of participation could have much more meaning for individuals and more of a potential impact on the Soviet system.

The available empirical data have added to the controversy. Unger (1981), relying on emigrant interviews, found support for the disengagement model: activists in the party and Komsomol were seldom enthusiastic about public activism or committed to the values of these organizations. Friedgut's (1979) research in the USSR and interviews with Soviet emigrants showed that people were uninterested in state-sponsored political activity. They acquiesced "passively but gracefully" in the post-Stalin regime's demands for a public display of activism, especially since the bulk of *obshchestvennaia rabota* (civic activity) could be carried out at work.

Friedgut (1981) also found, however, that people who participated in residential and neighborhood activities such as local improvement projects enjoyed a sense of community with their neighbors and valued the tangible benefits of their work. Also, citizens who contacted their local officials and deputies had opportunities for "cooperative and active" work within the system. Such individuals, in Friedgut's (1983) analysis, felt a sense of competence about their ability to get a response from local institutions. Similarly, Hahn (1987, 261) contended (based on interviews with deputies to local soviets) that "Soviet citizens, either as deputies or through them, can and do participate effectively in their local government."

Yet DiFranceisco and Gitelman's (1984, 610–11) interviews with Soviet emigrants led them to conclude that political involvement did not connote a positive orientation toward political institutions. Participants in political life were found to be hostile or at best neutral toward the Soviet system, distrustful of other people, lacking in political efficacy, and dubious about the value of formal participation. Indeed, the authors stood the normal connection between individual values and citizen participation on its head: it was the people most alienated from the regime and from other people who were the most "self-initiated and active."

The explanation DiFranceisco and Gitelman (1984, 610–11) offered for this striking result emphasized the social atomization of Soviet political culture: "The participation of Soviet citizens lacks an element of interpersonal trust that is essential to cooperative public activity." The authors thus concluded that the "Soviet citizen tends to avoid or denigrate cooperative activity and formal channels of interest articulation." Friedgut (1979, 313–14) offered a similar assessment, noting an inherent conflict between "the incessant pressure on citizens to join together in participatory groups, and the atomized social structure generated by the regime's demand for orthodoxy and by the control mechanisms of totalist community penetration." Social atomization has also been put forward to explain why there was so little opposition to the established political order (e.g., Brzezinski 1961; Zaslavsky 1982).

Thus, Western scholars remain divided over the connection, if any, between the attitudes and the behavior of the Soviet citizen under the "old regime." And the debate leaves several critical questions unanswered.

One question is how many people did in fact participate in political life. Mobilization theorists suggested that it was extremely important for Soviet leaders "that political participation be intense and almost all-embracing" to serve the goals of the regime (Brzezinski and Huntington 1965, 92). And if participation was so pervasive, differences in individual motivation could have little bearing on rates of political involvement. A person's interest in politics or sense of personal efficacy or values would simply not matter.

In fact, conventional participation was hardly universal. Soviet researchers from the 1960s onward reported that participation in *obshchestvennaia rabota* typically engaged between 40% and 50% of the adult population (Kurevin 1986; Volkov 1965). The *Pravda* all-union survey in 1977 found that 44% of adults engaged in such activity; an all-union survey in 1980–81, 53% (Voz'mitel' 1987, 41). Even in the best of circumstances participation in *obshchestvennaia rabota* often reached only 60%–65% (Artemov 1987; Baikova 1977; Nazimova 1987; Shibalis 1987; Toshchenko 1980), with leaders and activists comprising a much smaller group.[1] And general meetings at work, which would be especially difficult to avoid, usually drew only 60%–70% of the work force (Kliuev 1987, 43; Starchenko, Pustel'nik, and Chernysh 1988, 91–92).

According to one study conducted in the early 1980s, substantial numbers of Soviet citizens claimed little interest in, or time for, public activism (Kurevin 1986, 44).[2] Another study reported that 21% of adults in an all-union survey had *never* participated in *obshchestvennaia rabota* (Voz'mitel' 1987, 41). Similarly, research on the effectiveness of political propaganda showed that Soviet citizens often avoided attending lectures and discussions on political themes (Remington 1983; White 1980). And a careful examination of voting data revealed that even electoral turnout was far from total. Studies based on emigrant informants and on reported votes suggested that the real turnout was lower than the widely proclaimed 99% (Karklins 1986; Roeder 1989b; Zaslavsky and Brym 1978). Soviet scholars now admit privately that the turnout data may have been exaggerated.

Although the level of participation of Soviet citizens could be considered high by comparative standards, for most people involvement in public affairs was sporadic at best, and only a fraction of the population took a leading or active role. Despite the regime's emphasis on universal public involvement, people chose whether to participate, evade, or dissent (Friedgut 1979; Little 1976). And those who did participate confronted a further choice of how much to engage in a given activity. Soviet scholars showed that people differed substantially in the time and energy they devoted to public life (Iovchuk and Kogan 1972; Smirnov 1983). The degree of activism depended not only on people's objective social position but also on their values and subjective disposition to participate (Davidiuk and Korobeinikov 1986).

A second unanswered question in the participation debate is how best to characterize the different forms of political involvement. Scholars who emphasize mobilization and disengagement tend to speak of participation as a whole, without distinguishing among different types. Other scholars identify several forms of activism but disagree on how to conceptualize them. Friedgut (1979), for example, devotes separate chapters to elections, local soviets and deputies, and communal or neighborhood organizations. Little (1976) and Hough (1976) discuss alternate forms of activism ranging from voting to membership in community groups. And DiFranceisco and Gitelman (1984) identify three main forms of participation: "formal-ritualistic participation," "citizen-initiated contacts with official persons and institutions whose task is to represent, check up on and run interference for people," and "contacts over implementation (particularized contacting)."

However, most empirical studies focus on a single type of political activity.

Yet the varieties of participation were hardly equal in the demands and incentives they offered for citizen activism. Voting in noncompetitive elections would make little demand on the individual's time, energy, and political commitment and offer little reward other than certifying compliance with official norms of citizen behavior. Other forms of participation, such as seeking benefits from the *zhilotdel* (housing bureau) or *sobes* [*sotsial'noe obespechenie,* social security office] would demand resources—knowledge of the bureaucracy, time, persistence, and sometimes personal connections—to work the system for personal advantage. But citizens might expect tangible gains from their efforts.

Demands and payoffs would vary among different state-sponsored organizations as well. Attending trade union meetings probably would not demand much, since it was just a part of the workday. But being a leader in a housing commission would demand time, knowledge of the bureaucratic system, and (contrary to the disengagement model) cooperative skills, including interpersonal trust. It could also offer more in the way of benefits, ranging from getting a better apartment to deriving feelings of community or a sense of accomplishment over the commission's work (Friedgut 1979, 1981; see also Wilson 1973).

Of course, many forms of Soviet public activism left individuals little choice. Voting and Komsomol and trade union membership seem to fit into this category. Some civic obligations might be assigned outright or tied directly to career advancement. Party membership was not only a prerequisite for a wide variety of responsible posts; party members were also expected to be active in election work, *druzhiny* (citizen militias), and other special assignments (Friedgut 1979).[3] But other choices, such as housing and parents' commissions, civic improvement organizations, and preservation groups, were much less structured.

Furthermore, there were several varieties of nonstate or self-initiated activism apart from contacting officials with individual complaints or requests. Unsanctioned and informal groups existed well before Gorbachev's reforms (Alekseeva 1977; Sundiev 1987), along with more unconventional types of political activism ranging from producing and distributing *samizdat* (unauthorized publications) to engaging in strikes and protests. It is not clear, however, how participants in such unsanctioned activities differed from their more conventional counterparts except that dissidents were said to be the only ones whose political actions reflected their real attitudes (Shtromas 1984; Unger 1981).

A third point of controversy in the participation debate is whether citizens could feel efficacious in a system in which their influence over leadership selection and policy decisions was so minimal (Sharlet 1967; Unger 1981). Certainly, the political system marginalized citizen influence over such choices, and empirical evidence from the USSR in the mid-1980s suggests that people were not optimistic about their ability to influence government decisions.[4]

Yet individuals may also have a sense of *internal* efficacy—a more general sense of personal effectiveness in dealing with others or a sense of control over their social environment—that would make them more inclined toward political participation.[5] Moreover, participation itself might generate some sense of limited influence. While people might lack the power to change institutions or leaders, they might still enjoy the *relative* advantage of control over others. In a society where so many decisions come from above, the opportunity to be a "little boss" could draw people into minor political roles (Lewin 1985; Unger 1981).

Finally, the participation debate raised a fundamental question about the connection between political activism and interpersonal trust. Analysts who stressed disengagement noted a lack of trust among individuals, as theorists of totalitarianism had suggested (Arendt 1951; Brzezinski 1961). And the absence of interpersonal trust meant that people would be unlikely to cooperate in pursuit of collective aims unless they were impelled by social pressure or by careerism to join public organizations. More recent evidence, however, belies this picture of social atomization: the generation that came of political age at the height of the Stalin era retains a low sense of interpersonal trust, but generations that came of age after World War II display much greater faith in other people (Bahry and Silver 1987). Hence, the Soviet system under Brezhnev was not the atomized society that the mobilization model implied.

Reconceptualizing Soviet Citizen Participation

We suggest that measures of overall participation have limited value in explaining patterns of citizen activism and passivity. Instead, we offer a conceptual scheme that emphasizes the differences among alternate forms of participation, and the different mix of incentives (both positive and negative) that applied to each one.

A key distinction is whether individuals acted alone or in groups. Cooperative or group activity by definition suggests results affecting the wider community, while individual activism is likely to be more restricted in its scope (see Verba and Nie 1972). Joining a workers' soviet will have different implications from merely contacting the local housing office. Moreover, working within the group requires certain kinds of resources (such as faith in people) that are not necessarily relevant to more particularistic forms of political behavior such as contacting public officials. Contacting officials, especially in pursuit of individual material benefits, is typically episodic, requiring little in the way of cooperative behavior or interpersonal trust; and it is likely to be unrelated to a person's beliefs about the larger political system.

Another critical distinction is that between conventional and unconventional activism. We would argue that these are separate dimensions of participation, not simply different points along a single continuum (see Kaase and Marsh 1979). In other words, people might engage in both conventional and unconventional activity or neither. Political nonconformity entails a willingness to risk official retribution and public sanction that sets the participants off from those who are either active in conventional activities or merely passive conformists.

Conventional participation has many forms, however. Some activities, such as election work and involvement in citizens' militias and comrades' courts, have traditionally been more politicized than others. These are activities that the Communist Party has considered more critical (Friedgut 1979) and whose payoffs have generally been higher for individuals interested in advancing their careers. In contrast, participation in residential and neighborhood activities such as housing commissions represents a different, more "social" form of activism. The rewards are more likely to be purposive and solidary rather than career-based.

It is, of course, important to distinguish between simply joining public organizations and being active in such groups. While many people belonged in name only, many others took a more prominent role. Activists and nonactivists should differ markedly in their levels of interest in politics, trust in others, sense of personal influence, and commitment to the values of the Soviet regime.

We argue not only that individual atti-

Table 1. Schematic Diagram of the Expected Relations between Individual Orientations and Political Activism

	Cooperative Activity			Particularistic Activity
Orientations	Unconventional Activism	Conventional Political Activism	Conventional Social Activism	Contacting Officials or Media
Interest in politics	+	+	0	+
Subjective material satisfaction	−	+	+	−
Faith in people	+	+	+	0
Sense of personal influence	+/−	+	+	+
Support for regime norms	−	+	+	0

Note: The signs indicate the direction of the expected relationship: positive, negative, or none.

tudes ought to matter in determining how much people participated but also that a different combination of them would come into play for each type of activism. We are particularly concerned here with attitudes central to the participation debate: levels of interest in politics, sense of personal influence, commitment to regime norms, interpersonal trust, and satisfaction with material living standards. These do not exhaust all possible individual orientations to political life, but they offer a good test of our argument.

Table 1 summarizes the *expected* relationships. The sign indicates the expected direction of the relationship: positive, negative, or none.

We expect an individual's sense of personal influence to be an important political resource. Those who feel influential should be more involved in conventional activities, especially activities requiring cooperation. They should also be more inclined to contact public authorities, since they would be more confident about getting a positive outcome. In addition, having a greater sense of personal influence should be important for unconventional activism in either of two ways: on the one hand, people who feel influential might be less inclined to nonconformity, because they have less *reason* (i.e., they feel influential in the society the way it is); on the other hand, a greater sense of personal effectiveness might be an important resource that would allow someone to cope with the risks of unconventional activism.

Based on findings from other countries as well as the Soviet Union (Almond and Verba 1963; Bahry and Silver 1987), we expect individuals who have more faith in other people—who are higher in levels of interpersonal trust—to engage more in cooperative political activity, because they are more likely to be *able* to cooperate and to count on the support of others. Faith in people should also be positively related to involvement in *unconventional* activity, since an ability to trust others reduces the perceived costs of being unconventional (Bahry and Silver 1987). But there should be no correlation between faith in people and citizen-initiated contacts with officials, since this activity does not require cooperation among citizens.

Having more interest in politics should lead to greater unconventional activity and cooperative conventional activity. It should also raise the odds of contacting public officials, since those with high levels of interest in the political world would be more inclined to view the government or party as an appropriate avenue for resolving individual problems. Satisfaction with one's material quality of life (housing, job, standard of living) should give people a tangible stake in the system, thus fostering conventional political and community activism and inhibiting unsanctioned political activity. Conversely, those who are dissatisfied with the material quality of life should be more inclined to contact officials or the media, since they would have more reason to turn to public officials for assistance.

Finally, people who concur with fundamental values of the Soviet regime (i.e., state control of the economy and government limitations on individual civil rights) should be more involved in compliant political and social activity, since they would have more of a normative stake in the system; while those who oppose these traditional regime norms should be more disposed toward unconventional activity. However, we expect that support for regime norms will be unrelated to contacting of officials, because this activity is typically personalized and nonideological.

In sum, there should be a *psychology* of Soviet political participation, not simply a sociology. Contrary to the image of apolitical participation under Brezhnev, individual attitudes and motivations should make a difference. And the particular mix of attitudes that foster activism should vary with different types of participation. This is not to say that activists were satisfied with the system or content with the existing avenues for political participation—far from it.[6] But individual differences were more important in accounting for the extent and types of participation than the notions of mobilization and disengagement have allowed.

Data

The data for this study come from several sources. We rely primarily on the Soviet Interview Project (SIP) general survey, with supplementary material from discussions with Soviet specialists on political activism and from personal interviews with several activists in new informal groups during fall 1989. In the SIP survey, the 2,793 respondents are Soviet emigrants who arrived in the U.S. between 1 January 1979 and 30 April 1982.[7] Most of the questions focused on a respondent's "last normal period" (LNP) of life in the USSR, that is, the five years before his or her life changed significantly in connection with the decision to emigrate. For most respondents, the end of the LNP occurred very shortly before they actually emigrated. Thus, the responses refer to the end of the Brezhnev era. For this analysis, we restrict our attention to the 2,667 respondents who were age 18 or older at the start of their LNP.

The SIP sample has been described in detail elsewhere (Anderson and Silver 1987a), and potential problems of bias in a sample of this nature have been addressed at length (Anderson and Silver 1987b; Bahry 1987; Millar 1987; Silver 1987; Swafford et al. 1987). The three most significant potential sources of bias are that most of the respondents (about 85%) are Jewish, that all are emigrés, and that they come primarily from large cities and have above-average education and incomes compared to the Soviet population as a whole. The potential for bias from these sources would seem to be particularly large for questions on attitudes and political participation.

We cannot summarize here the many studies testing for bias in the SIP data. It is important to note, however, that the biases do not affect the basic findings we report. After examining the effects of ethnic and emigration bias and controlling for the effects of social status, we are satisfied that our conclusions hold up regardless of the nationality of the respondent, the role the respondents played in the decision to leave, and the social composition of the sample.[8]

Equally important, new survey data emerging from the USSR confirm many of the results from studies based on SIP data.

For example, the basic connections found in the SIP survey between education, generation, and gender on the one hand and satisfaction with the quality of life on the other are almost exactly mirrored in a survey of residents of Moscow in 1988 (*Project Understanding* 1988). Even more compelling data (for the current study) emerge from a comparative study of participation among U.S. and Estonian youth (Silver and Uueküla 1989) showing a strong relation between individual values and conventional political participation among young adults in Estonia in 1987. Unfortunately, new surveys still do not touch on many topics critical to our understanding of political change in the Soviet Union and shed no direct light on patterns of political behavior before Gorbachev's drive toward democratization.

The value of the SIP interviews for the present study lies in the opportunity to test some critical hypotheses about political engagement and disengagement under Brezhnev. In fact, the data offer a particularly stringent test of our argument. If we assume that political disengagement ought to be higher among emigrants than among the general Soviet population, positive evidence of political engagement in our sample is all the more compelling. Nonetheless, in view of the social backgrounds of the respondents, the SIP data should not be assumed to reflect the views of the rural population or of non-European populations in the Soviet Union. Instead, the appropriate Soviet referent population is the "adult European population in large and medium-sized Soviet cities" (Anderson and Silver 1987a, 361).

To gain a better understanding of the new forms of political organization emerging under Gorbachev, one of us (Bahry) also interviewed several leaders of "informal groups" in Moscow and Tallinn in October and November 1989.[9] The interviewees were selected to reflect a broad range of the political spectrum—from those demanding market reforms and full democratization to those calling for a return to traditional values and restoration of Marxism. While such a small sample is limited in what it can tell us about the full scope of informal group activities, it does allow us to raise some questions about how participation may be changing under Gorbachev.

Results

We begin by describing the varieties of political participation by SIP respondents in their LNP. Using factor analysis, we divide the different kinds of participation into four categories, then analyze the relation between individual attitudes and the degree of involvement in each type of activism. This analysis both clarifies the structure of political participation in the Brezhnev period and makes clear the need to reappraise previous generalizations about individual motives for political activism.

Varieties and Levels of Political Participation

Tables 2–4 show the number of people who engaged in each of several activities during their LNP.[10] As we would expect, of all the varieties of participation that respondents discussed, voting and trade union membership were the most common. Nearly 90% of the respondents reported that they voted at least sometimes during their LNP, and a similar percentage belonged to a trade union. Of those who did vote, however, only four-fifths *always* voted; and of those who belonged to a trade union, fewer than half attended meetings regularly. Thus, the two most pervasive forms of political activity included a large segment of marginally mobilized people, as we would expect when trade union membership was all but mandatory and when election workers might bring a ballot to the house to ensure a high turnout.

Table 2. Incidence of Political Activity during Last Normal Period of Life in the USSR

Type of Activity	%	N
Voted[a]		
Always	69.8	
Sometimes	19.1	
Never	11.1	2,661
Contacted party or government official[b]	19.4	2,667
Worked in electoral campaign	12.1	2,667
Contacted media[c]	4.2	2,666
Involved in Komsomol in LNP[d]		
Leader	19.4	746
Member	60.9	746

[a] "Did you yourself always vote in elections to the soviets during your last normal period, or did you sometimes not vote?" The table excludes those who were not eligible to vote in their LNP. People who reported that someone else cast a ballot for them were counted as not voting.

[b] "During your last normal period, did you ever go to see or write to a government or party official about some need or problem *not* related to your emigration?"

[c] "Between [DATE 5 YEARS BEFORE END OF LNP] and [END OF LNP], did you ever, on your own initiative, write a letter to or call a local, republic, or central newspaper, journal, or radio or TV station?

[d] "Did you ever belong to the Komsomol?" The figures on Komsomol membership refer to respondents who were between ages 18 and 32 inclusive at the end of their LNP. In the sample *as a whole,* out of 2,657 respondents, 17.1% had been Komsomol members and another 5.5% had been Komsomol leaders in their youth.

Other types of public activism reveal a smaller gap between formal membership and actual involvement. Most of the people who belonged to housing commissions, people's control, party and soviet committees, and similar public groups report that they actually participated in those organizations, though the frequency of their participation varied.[11] Of those who belonged to a committee or commission at work 98% participated at least sometimes, and 71% did so regularly. In contrast, although 92% of those who belonged to a citizen's militia or a comrades' court participated at least occasionally, only 42% did so regularly.

Overall, 20% of the SIP respondents were leaders or activists in at least one public organization (including the Komsomol), and another 50% were members of at least one such group. Excluding rank-and-file trade union membership, roughly half of the respondents were either leaders or members of one or more public organizations, well within the range indicated in Soviet surveys. Also, 12% worked in election campaigns, and about 20% contacted either the media or a party or government official during their LNP.

Few people engaged in unconventional activity such as unsanctioned study and discussion groups, protests, and strikes: 10% took part in at least one such activity during their LNP, and another 2% were leaders.[12] The most common form of unconventional activity was participation in study or discussion groups. Very few people took part in protests or strikes.

Dimensions of Political Participation

Although it is possible to rank individuals according to their level of involvement in both conventional and unsanctioned political activity, using simple scales of activism begs the question whether certain types of activity tend to group together. Were those who engaged in the more common types of activity such as voting but did little else, just less involved in political life in general? Or were they distinct in other ways from those who engaged in alternative forms of public activism?

To group the different forms of participation and to allow comparisons with cross-national empirical research on participation, we rely on factor analysis. Table 5 presents the results of a factor analysis of the activities listed in Tables 2-4.[13] The activities group along four dimensions: (1) *unconventional political*

Table 3. Incidence of Other Conventional Activities during Last Normal Period of Life in the USSR (%)

Type of Activity	Leader	Went Regularly	Went At Times	Never Went	Did Not Belong	N
Commission at work	3.0	2.9	2.2	.2	91.6	2,652
People's militia or comrades' court[a]	1.3	3.7	5.9	.9	88.3	2,657
Party or soviet commission	.3	.2	.3	.1	99.1	2,661
Trade union	9.4	31.6	34.0	12.5	12.6	2,652
Other public organization[b]	1.4	2.9			95.7	2,667
Housing commission	.9	.6	.9	.0	97.5	2,660
Parents' commission	2.4	5.0	3.2	.0	89.4	2,660

Note: "I'd like to know something about the groups and organizations that you belonged to during your last normal period in the Soviet Union. During that time, did you belong to . . . a trade union? a housing, sanitary or repair commission? a parents' committee? a volunteer committee of the local soviet or party organization? the People's Militia, People's Control or a Comrade's Court? committees at work? [Respondents who said they belonged to such organizations were asked, for each item, how often they attended and whether they were a leader, organizer, or officer in the group.] Was there any other public organization [*obshchestvennaia organizatsiia*] that you regularly took part in then?"

[a] Also includes participation in People's Control posts and groups.

[b] Including, for example, professional soviets, DOSAAF, preservation societies, scientific-technical societies, and other groups.

Table 4. Incidence of Unsanctioned Political Activity (%)

Type of Activity	Leader	Participated Only	Did Not Participate	N
Belonged to study or discussion group	.6	6.5	92.9	2,665
Protested	.2	1.0	98.8	2,661
Struck	.7	.9	98.0	2,663
Distributed samizdat or tamizdat	.5	4.1	95.4	2,663
Engaged in other self-initiated activity[a]	.5	1.5	98.0	2,664

Note: "During your last normal period, did you . . . take part in an unsanctioned study or discussion group? distribute samizdat or tamizdat? take part in an open protest against some Soviet policy? take part in a strike or protest at work? belong to any (other) unofficial groups or take part in any (other) unsanctioned activities?" Respondents who said they took part in such groups or activities were asked, for each item, whether they took a leading role in them.

[a] Includes unofficial cultural and literary groups, religious and ethnic observances, writing of samizdat material, unofficial human rights groups, and other such activities.

Table 5. The Dimensions of Political Activism: Factor Analytic Results

Type of Activity	Unconventional Political Activism	Compliant Activism[a]	Social Activism	Contacting
Distributed samizdat or tamizdat	.740	.029	-.003	.048
Other self-initiated unsanctioned activity	.677	-.022	.014	-.068
Study or discussion group	.667	.052	-.017	.062
Protest	.658	-.059	.130	.018
Nonvoting	.441	-.049	-.211	.249
People's militia	.018	.694	-.012	-.039
Party or soviet commission	.030	.590	-.217	-.135
Election campaign	-.071	.518	.100	.113
Commission at work	.041	.502	.310	.091
Trade union	-.101	.468	.324	.102
Other public group	.053	.352	.206	.208
Parents' committee	-.018	-.016	.779	.029
Housing commission	.037	.161	.504	-.044
Contacted media	.050	.052	-.017	.740
Contacted official	.062	.073	.030	.702
% of variance	14.4	13.2	7.3	6.9
Eigenvalue	2.162	1.984	1.101	1.034
N	2,633	2,633	2,633	2,633

Note: The factor results were derived through principal components; the data in the table are standardized regression weights for each variable on each factor. The factor results were rotated using the varimax procedure in SPSS^x. The steps in the analysis are described in the Appendix.

[a] For the factor analysis, respondents who said they "never went" to a group to which they nominally belonged were treated as nonparticipants. An analysis run with them included as minimal participants yields the same basic results.

activism, incorporating distribution of samizdat, participation in public protests, involvement in unsanctioned study or discussion groups or other unsanctioned political activity, and avoidance of voting; (2) *compliant political activism,* defined best by involvement in activities that are the epitome of conventional behavior, such as serving on a commission or committee at work, a party or soviet commission, or a comrades' court or in the people's militia; (3) *social activism,* including groups and activities focused on neighborhood and family life, such as service on parents' committees and housing commissions; (4) *contacting,* defined by individual efforts to contact party and government officials and mass media—most involving requests to solve personal problems (such as housing repairs and pension benefits), a few involving complaints about government policies or the performance of officials (Bahry and Silver 1990).

Bear in mind that these groups of activities are not mutually exclusive. For example, most social activists also participated in some form of what we have called compliant political behavior. And some people, especially within the younger generations, engaged in both conventional and unconventional political activity.

Nonetheless, the analysis so far bears out our expectation of several distinct dimensions of political participation. The question remains whether the four types of activity attract people who have different subjective political orientations.

The Bases of Political Activism: A Multivariate Analysis

For each broad category of political activism we estimate a separate regression equation to capture the effects of individual attitudes. Each equation also includes a series of social background and status measures to allow us to evaluate the subjective side of political involvement independently of a person's education, gender, age, and other personal characteristics. The results are presented in Table 6.[14]

The multiple regression results show that individual motivations did make a difference: people who were less satisfied with their material quality of life, very interested in politics, less supportive of state control of the economy, or had high faith in people were more likely to engage in unconventional activity (col. 1 of Table 6). Thus, unconventional political behavior is clearly related not only to people's social background but also (independently of social background) to their political orientations.

It would be surprising had we not

Table 6. Multiple Regression of Four Types of Activism by Social and Demographic Characteristics, Interest and Influence, and Political Values

Independent Variables	Unconventional Political Activism	Compliant Political Activism	Social Activism	Contacting
Generation[a]				
1919–25	.036	.318**	.176	–.177
1926–40	.133	.215*	.550**	–.113
1941–60	.231**	.134	.099	–.251**
Education				
Completed secondary	.008	–.096	–.041	.167
Completed higher	.196**	.304**	–.085	–.045
Female[b]	–.104*	–.216**	.283**	.137*
Earnings[c]	–.029	.082	.026	.101**
Privileges[d]	.129*	–.075	.324**	.176**
On party *nomenklatura*[e]	–.253*	.786**	.115	.016
Sense of personal influence[f]	–.001	.194**	.182**	–.016
Interested in politics[g]				
Slightly	–.070	.106	.065	.063
Somewhat	–.083	.309**	.033	.241**
Very	.315**	.443**	.113	.312**
Faith in people[h]	.062**	.089**	.036	–.028
Satisfaction with material quality of life[i]	–.132**	.033	.075*	–.171**
Support for state control on civil liberties[j]	.005	.065**	.010	–.052**
Support for state control of the economy[k]	–.074**	.036	.027	.027
Constant	.153	–1.454**	–1.000**	–.518*
Adjusted R^2	.134	.132	.104	.050
N	786	786	786	786

Note: Some questions, such as those on interpersonal trust, were asked only of a random one-third of the SIP sample, making our effective sample size (with missing data excluded) 786. The coefficients are unstandardized. The dependent variables are factor scores with a mean of zero and a standard deviation of one. We also ran the same models a second time to include all the variables listed plus controls for respondent's ethnic group (Jewish or non-Jewish) and role in the family's decision to emigrate from the USSR. The results prove to be the same: the connections between political action, background, and attitudinal factors hold whether respondents

TABLE 6 (*continued*)

were Jewish or not and whether they made the decision to leave the USSR themselves or merely accompanied other family members.

$^{*}p \leq .10$.
$^{**}p \leq .05$.

[a]These generational categories are derived from Jerry F. Hough, *Soviet Leadership in Transition* (Washington: Brookings, 1980).

[b]*Female.* One if respondent was female, zero if male.

[c]*Earnings.* Natural log of the monthly salary at the last job before the end of the LNP.

[d]*Privileges.* A count (from zero to three) of the individual's access to special benefits, e.g., access to a state car, a closed medical clinic, or a special store.

[e]*Nomenklatura.* One if respondent held a party *nomenklatura* position, zero if not.

[f]*Sense of Personal Influence.* "Here is a ladder. Let's say that the *most influential* person in the Soviet Union is at the top and the person with the least influence is at the bottom. Where would you have put yourself on this ladder in the Soviet Union in terms of your influence?" Probe further, if necessary: "Would you have placed yourself near the most influential person at the top or near the person with the least influence at the bottom?" Scores range from 1 (lowest) to 10 (highest).

[g]*Interest in Politics.* "During your last normal period, how interested were you in politics and public affairs—were you very interested, somewhat interested, only slightly interested, or not at all interested?" The categories are rescored as dummy variables.

[h]*Faith in People.* Two items are used to construct the faith in people measure: (1) "Some people we've talked to say that most people can be trusted. Others say you can't be too careful in your dealings with people. How did you feel about that during your last normal period?" (*Most people can be trusted, You can't be too careful, It depends* [if volunteered]); (2) "Speaking generally, would you have said during your last normal period that most people are more inclined to help others, or more inclined to look out for themselves?" (*More inclined to* help others, More inclined to look out for themselves, It depends [if volunteered]). Scores are derived by assigning a 3 to the answers *Most people can be trusted* and *More inclined to help others,* 1.5 to It depends, and 0 to *You can't be too careful* and *More inclined to look out for themselves,* then averaging the values of each item. If only one item was answered with a valid value, only that item is used to compute the score.

[i]*Satisfaction with Quality of Life.* Based on average responses to five items. "In [end of LNP], how satisfied or dissatisfied were you with . . . (a) your housing, (b) [your/your family's] standard of living, (c) public medical care, (d) your job, (e) the availability of consumer goods in your town? Response categories: 2, *very satisfied,* 1, *somewhat satisfied,* −1, *somewhat dissatisfied,* −2, *very dissatisfied.* The scores range between +2 (*most satisfied*) and −2 (*least satisfied*), with 0 as a midpoint.

[j]*Support for State Control of Civil Liberties.* The average response to three items: (1) "Some people in the Soviet Union say that the rights of individuals accused of crimes must be protected even if a guilty person sometimes goes free. Others say that the rights of society must be protected, even if an innocent person sometimes goes to prison"; (2) "Some people in the Soviet Union believe that workers should not be able to strike, because strikes are costly. Other people feel that all workers should have a right to strike, even if it means that certain services may be interrupted"; (3) "Some people in the Soviet Union believe that people should be required to have residence permits to live in the large cities so that the authorities can plan public services. Others think that people should be completely free to live where they want." For each opinion part, respondents rated themselves on a seven-point scale, where one represents no state controls, and seven represents state controls.

[k]*Support for State Control of Economy.* Average response to three items: (1) "Some people in the Soviet Union say that the state should provide free medical care for all citizens. Others believe that medical care should be provided and paid for privately"; (2) "Some people in the Soviet Union say that the state should own all heavy industry. Others say that all heavy industry should be owned privately. Where would you have placed yourself on this issue in [end of LNP]?" (3) "Some people in the Soviet Union believe that the state should control production and distribution of all agricultural products. Others believe that all agricultural production and distribution should be private." For each question, respondents placed themselves on a seven-point scale, ranging from one (no state control) to seven (full state control).

found such relationships. Given the potential for an adverse response by political authorities, political nonconformity *ought to* be more common among those who hold clear motivations to violate political convention. But the disengagement model implies that individual motivations should bear little relation to conventional forms of political participation. Since conventional activism tends to be viewed as mobilized participation, those who become involved need not be favorably disposed toward the regime, satisfied with the material qualify of life, interested in politics, or trusting of other people.

The evidence, however, suggests a rather different picture. Compliant political activism (involvement in commissions at work, for the party, or for soviets and involvement in trade unions or other public organizations) was indeed related to an individual's attitudes and values (col. 2 of Table 6). Even with social background taken into account, people with a stronger sense of personal influence, greater interest in politics, greater faith in people, and support for state control over civil liberties were more involved in conventional political activities.

Social activism also depended on individual motivations (see col. 3 of Table 6). People who felt more influential and more satisfied with their material quality of life were more inclined to be social activists. But the more-political motivations seem to have been less important: social activists did not have especially high levels of interest in politics or support for state control over civil liberties and the economy.

Whether individuals contacted public officials or the mass media (col. 4 of Table 4) also depended on their attitudes. People who were less satisfied with their material quality of life, more interested in politics, or less supportive of state control of civil liberties were more likely to contact an official about some problem or issue. But there was no relationship between individual contacting and the amount of faith in people. This is just what we would expect, because *individual* contacting (the seeking of particularistic favors or services from public officials) would not necessarily require cooperation between individual citizens.

Thus, subjective motivations made a difference in political activism independently of objective social status.[15] The empirical results are quite consistent with the expected patterns depicted in Table 1. Although some of the findings do not fit the anticipated pattern, in no case is there a reversal of signs between our initial expectation and the empirical results.

One unanticipated finding is that satisfaction with material living standards did not lead to greater conventional political activism. We think this may be due to the asymmetric effect of subjective material satisfaction: people who were satisfied might be either active in regime-sponsored organizations or simply passive, while those who were *dissatisfied* would be motivated to contact the authorities or to engage in unconventional activism. A second unexpected finding is that neither faith in people nor support for regime norms was linked to the degree of social activism.[16] This does not mean that such activists were disengaged or lacking in real motivation; rather it suggests only that political motives were less important than others in fostering such participation.[17] As Friedgut (1979) observed, people derived other kinds of satisfaction from involvement in community affairs, such as a sense of communal identity.

Third, the connection between support for regime norms, unconventional activism, and conventional activism turns out to be more complex than we expected. It is not simply the case that conventional activists were "for" state control while unconventional activists were "against" it. For one thing, conventional and unconventional activism were not mutually exclusive: some people, especially among the youngest generation, engaged in both. Nor were the two dimensions of state control that we examined (control over the economy and control over individual behavior) perfectly correlated. Respondents proved far more willing to accept government ownership and central planning than restrictions on civil liberties.

Fourth, we had anticipated that a stronger sense of personal influence would mean greater odds of contacting the authorities, but we found that subjective personal influence had no effect on this form of participation. The reason may be that people who felt influential had other, less formal ways of getting things done, such as relying on connections to handle problems and complaints. Additional data in the survey support this interpretation: those who felt they had good connections were less inclined to contact the authorities themselves (data not shown).

Finally, we had expected that there would be no connection between support for regime norms and contacting public officials. Yet our results showed that those who favored broader civil liberties were more inclined than others to contact a party or government agency. Further analysis reveals, however, that the connection holds only for certain types of contacts. Advocates of expanded civil liberties were more likely to turn to a government or party organization for what might be called political reasons, such as complaints about official behavior or state policy, but they were exceptions: they made up a distinct but very small percentage of all contacts. Those who went to officials to obtain material benefits (who accounted for the vast majority of contacts) were just as supportive of state control as those who did not. Therefore, the typical "covert participant" was no more and no less alienated from the regime than other people.

We should also note that although our findings on conventional political participation and subjective influence are in line with our earlier predictions, one might question the direction of causality. A stronger sense of personal efficacy can lead to greater participation, but activism can also *give* people a sense of influence. Or both could simply be functions of a person's social or job status. After all, a show of activism was expected of people aspiring to managerial or administrative posts.

We would argue that the connection between participation and influence is reciprocal. People who lack even a minimum feeling of personal effectiveness are likely to remain passive. It may take some sense of individual influence to become actively involved in public life (beyond simple membership in an organization). And becoming active in public life (with the opportunity to exert authority over other people) is also likely to strengthen a person's sense of influence in society at large even though it may not yield a sense of being able to influence leaders or policies. In fact, Soviet survey research suggests that acquiring some authority over others can be a prime motive for participation.[18]

Our data indicate, however, that the degree of activism and the organizational context made a difference in how influential people felt. When participants were asked whether "people like you" had any influence over the group's activities, the more active they were, the more they felt that activists had influence (Table 7).[19] The type of organization mattered as well. Activists in social organizations—organizations with fewer party members—were the most inclined to feel that people like themselves could affect the work of their group. Activists in more overtly political organizations, such as the party and the soviets, felt their groups offered fewer possibilities to wield influence.

The connection between activism and sense of influence is not simply a product of higher social status. As Table 6 shows,

Table 7. Members' Perceived Sense of Influence on Conventional Public Organizations

Type of Organization	Leader	Regular Attendee	Occasional Attendee
	Percentage Saying "People Like Me Had a Say"		
Parents' commission	83.6 (61)	64.7 (133)	57.1 (84)
Committee or commission at work	66.3 (80)	54.4 (79)	43.1 (58)
Housing commission	64.0 (25)	64.7 (17)	50.0 (24)
People's militia or comrades' court	58.8 (34)	20.6 (97)	21.4 (154)
Commission of local party or soviet	50.0 (8)	57.1 (7)	12.5 (8)
Trade union	42.7 (246)	31.0 (828)	20.6 (887)
Other organization or group	81.1 (37)	37.0 (73)	—[a]

Note: This question was asked only of those respondents who said they went at least sometimes to one of the organizations listed. The wording was, "Did people like you who (regularly) attended meetings (and were active in the organization) have a say in deciding group activities?" (*Yes/had a say, No/did not*). Number of cases are in parentheses.

[a] Only respondents who reported that they attended regularly or were leaders were recorded in the survey as belonging to an "other organization."

subjective personal influence is linked to conventional participation independently of one's level of education, income, and privileges. Other data (not shown) bear out this conclusion as well: even among people at the same rank in the occupational hierarchy, those with a greater sense of personal influence were more active in public organizations.

Varieties of Disengaged Participation

Our empirical investigation calls into question several key elements of the mobilization model. The image of undifferentiated, ritualistic participation in state organizations clearly did not fit the pattern of political activism under Brezhnev. Nor is it accurate to picture the Soviet citizen then as psychologically disengaged, mobilized to take part in public life by a simple desire to get ahead or to avoid conflict with the authorities. Individual motivations and attitudes mattered, not only for unconventional activities but also for conventional ones. People made choices whether to participate in most types of activity, choices that reflected their attitudes as well as their social position.

Nevertheless, the disengagement model did apply to some forms of participation. Enforced turnout and lack of choice among candidates meant that voting was usually not related to an individual's level of interest in politics, sense of personal influence, or faith in people. An analysis of the correlates of voting similar to that presented in Table 6 confirms this (see Table 8). Still, *non*voting does seem to have been a form of protest (see also Karklins 1986). Not only does it cluster with other forms of unconventional political activism in the factor analysis, but (as shown

Table 8. Multiple Regression of Voting by Social and Demographic Characteristics, Interest, Influence, and Political Values

Independent Variables	Unstandardized Regression Coefficients[a]
Generation	
1919–25	.115
1926–40	–.008
1941–60	–.063
Education	
Completed secondary	–.131*
Completed higher	–.129*
Female	.138**
Satisfaction with material quality of life	.139**
Earnings	–.033
Privileges	–.025
On party *nomenklatura*	.033
Sense of personal influence	.045
Interested in politics	
Slightly	.086
Somewhat	.076
Very	–.113
Faith in people	–.021
Support for state control of civil liberties	.020
Support for state control of the economy	.067**
Constant	-1.610**
Adjusted R^2	.145
N	793

[a] Voting is a three-category variable. Of people who were eligible to vote at the end of their LNP, 1 = *never voted,* 2 = *sometimes did not vote,* and 3 = *always voted.*

$^{*}p \leq .10$
$^{**}p \leq .05$

in Table 8) people who were less satisfied with their material quality of life and who were less supportive of regime norms voted less.[20]

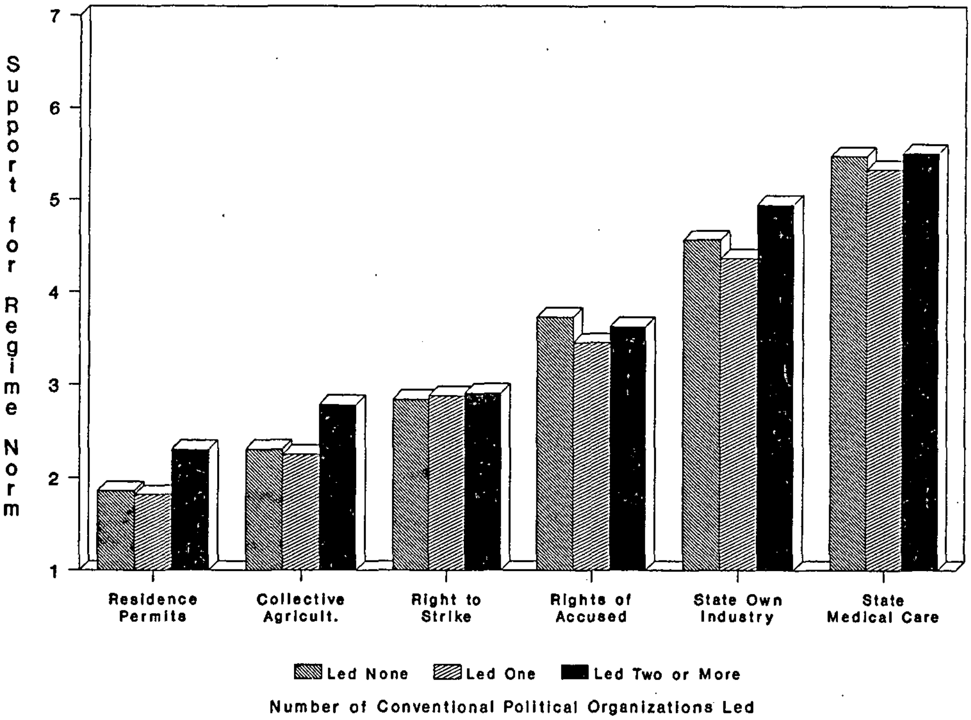
Komsomol membership is another activity to which the disengagement model applied. Whether young people were Komsomol members during the LNP depended on neither their social background nor their political attitudes.[21] The main reason is that like voting, Komsomol membership was so widespread among the target population—so lacking in selectivity—that being a member said little about whether an individual was psychologically engaged or disengaged from political life.[22]

In contrast, being a party member said a good deal about whether a person was psychologically engaged in political life. Using a proxy measure for party membership, we find that people who were more interested in politics, felt more influential, and who were more supportive of state controls over civil liberties were more likely to be party activists.[23] This is not to say that party activists accepted the party's style of rule or its leaders or programs uncritically (Bahry and Silver 1990). But their subjective orientations to politics were indeed different from those of nonactivists (see also Zaslavsky 1982, 32–33).

To find that activists in the party and in other conventional political organizations were more "engaged" leaves us with a puzzle, however. If the more active citizens were indeed the more interested, the more trusting, the more efficacious, and the more supportive of core regime values, how can one explain the mass exodus from conventional organizations in the era of *perestroika?* We would suggest several answers (while emphasizing that "old" organizations have varied in their responses to *perestroika* and democratization). Although some marginal participants may well be activated by the new political atmosphere under Gorbachev, many (especially the most marginal) now have more opportunity to opt out of organized political life in favor of other, nonpolitical pursuits.

On the other hand, those who were most active under the old system have acquired the opportunity to leave restrictive, hierarchical organizations for groups whose agendas and style offer a closer fit to their own values. Political competition has thus drawn the activist stratum into new channels. As one scholar at the

Figure 1. Support for Regime Norms by Status As Leader of Organization



Source: SIP G1 Survey.

Note: 7 = maximum possible state support, 1 = minimum possible.

Academy of Social Sciences told us, many activists in the Brezhnev era had untapped "reserves" of commitment and motivation that have been allowed fuller play under Gorbachev (on this same theme, see "Samodeiatel'ny initsiativy" 1988).

In addition, even though activists in conventional political organizations were on average more supportive of the regime's values than were nonparticipants, they were by no means in accord with the regime's position on many key points. As Figure 1 reveals, leaders in conventional political organizations were generally favorable toward state ownership and control of heavy industry and (especially) state provision of medical care.[24] But they were substantially less enthusiastic about the state's restrictions on the right to strike and about the use of residence permits to regulate migration to cities. And they were strongly inclined to support private, rather than state-run, agriculture.[25]

Implications

Our results suggest the need to reevaluate some key issues in the debate about the meaning of political participation in the USSR on the eve of the Gorbachev era. The portrait of disengaged participation, of a disparity between personal beliefs and public behavior, is both true and false. Some people *were* uninterested in politics, distrustful of others, and skeptical about core regime norms on the role of the state. And a great many felt that they lacked influence: only a quarter of SIP respondents rated themselves as having even moderate influence in Soviet society; and Soviet survey data as recently as 1987 suggested that levels of external efficacy—of feeling able to influence government or party policies—were even lower (*Politicheskoe soznanie* 1988, 37). Soviet research also confirms that at least half of the adult population often did not take an active role in public life. Political participation in the Brezhnev era drew in a much narrower stratum of activists than Soviet theory prescribed.

These are facts about the *extent* of interest, efficacy, trust, and participation. They say little, though, about the *connection* between attitudes and activism. Some types of conventional behavior, namely, the most pervasive ones such as voting and Komsomol membership, bore little relation to individual values and attitudes; but other forms of conventional activism did. The more interested in politics people were, the more influential they felt; and the greater their faith in others, the more they participated in conventional political activity. Citizens who were psychologically disengaged were significantly less active.

One explanation for this selectivity in real participation would focus on the system itself, emphasizing the failure or decline of regime-directed mobilization (Roeder 1989a; Zimmerman 1987). The system had relaxed in many ways, coercion had diminished, and people were less malleable. Another explanation, however, would emphasize the importance of individual characteristics. People differ in their psychological involvement in politics and in the relevance of political life to other everyday concerns.

We would argue for a synthesis of both views. Certainly, no analysis of participation can ignore the regime's efforts to structure and limit citizen involvement in the political process. Yet the empirical evidence shows that individuals responded in different ways despite the state's quest for universal participation in officially approved activities. Thus, no explanation of citizen involvement can be complete without recognizing the differential incentives and resources that led people to choose given paths.

Our analysis also suggests the need to rethink the results of earlier Western studies of Soviet political participation. We have shown that participation actually comprised several different forms of activism, each of which mobilized a somewhat different segment of the population. In this sense our results mesh with those of authors who emphasized the distinction among different types of political behavior (Friedgut 1979; Hough 1976; Little 1976). We have, however, offered an alternate conceptualization of political participation, one based on the costs and consequences of different political acts for the individual. We also find a clear correlation between values and behavior, particularly conventional political involvement, in contrast to the image of a Soviet citizenry acquiescing "passively but gracefully" in the regime's demands for a show of activism (Friedgut 1979; Shtromas 1984).

Yet our empirical results do suggest disengagement in particular types of political participation. For example, our findings parallel Unger's (1981) conclusion that many Komsomol members were disengaged. But we would suggest that party membership should not be lumped into the same category. As we have shown, the party recruited people whose subjective orientations to politics better fit the model of a conventional activist.

We also found, as DiFranceisco and Gitelman (1984) emphasized, that citizen-initiated contacts with government and party officials represent a conceptually distinct form of political involvement; and like those scholars, we found a lack of a connection between this type of participation and interpersonal trust. But our analysis does not support DiFranceisco and Gitelman's conclusion that *political participation* lacked a critical element of faith in people or that it was devoid of emotional content. We have shown that *contacting of public officials*—one form of political participation—was often devoid of much emotional content.[26] Most people who turned to the government or the party did so to resolve very specific material problems that had little to do with their ideological orientation or their level of interpersonal trust. They were often dissatisfied with material conditions and so sought assistance from public agencies. But other forms of public involvement reflected a different set of orientations: The most active citizens in conventional politics were more trusting of other people, more supportive of key regime values, and more positive about their own influence in public organizations and in society at large.

Our analysis also puts political events under Gorbachev into perspective. Contrary to the disengagement model's emphasis on a socially atomized society, many Soviet citizens in the Brezhnev era did in fact trust other people and did value cooperative activity. That would help to explain how a purportedly atomized political culture could suddenly give rise to thousands of informal groups under Gorbachev. Of course, the rise of new groups highlights the lack of outlets under the old system for a wide variety of political interests, some of which were expressed as dissent or unconventional participation under Brezhnev.

Our findings also hold some implications for analyzing changes in citizen participation from Brezhnev to Gorbachev. The existence of an interested, activist stratum in the Brezhnev era implies that the USSR experienced a form of political specialization by its citizens common in industrial societies, despite the regime's stage-managing of mass participation. The system drew people with activist orientations into public roles—people whose higher levels of interpersonal trust, sense of personal effectiveness, interest in politics, and support for key regime values set them apart from the average citizen. The system also created situations in which activists could acquire political experience and skills—if only in cajoling their less committed coworkers and neighbors into participating nominally in political life.

We would expect that while the scope and forms of mass participation have changed with democratization, the pattern of political specialization we have described under Brezhnev would persist. At the mass level the opening up of the electoral process alone has enabled a large segment of the population who formerly voted as a show of good citizenship to become more engaged in the system. The March 1989 defeat of many official candidates to the Congress of Deputies who were running unopposed suggests that Soviet citizens no longer regard voting as a formality. Moreover, hundreds of thousands of people have now taken part in strikes or public demonstrations. As the literature on transitions to democracy reminds us, the first wave of democratization produces a temporary surge in political activity as old constraints are removed. But the number of people with an ongoing commitment to political activism is likely to be very limited. As a more voluntary mass politics develops in the Soviet Union, one can expect a substantial proportion of the population to avail themselves of new opportunities *not* to participate in politics despite the efforts of leaders of new political organizations to mobilize the population behind their banners and programs.

If our argument is correct, people who were active before democratization should be overrepresented among the "new" activists. The number of "gladiators" in the political arena should remain a minority. Once the transition period ends, most people would play the role of "spectators," following and discussing politics and also voting, but seldom going beyond this level of political activism.[27]

Interviews with Soviet sociologists and with leaders of new informal groups themselves suggest that the number of activists is indeed limited. By their estimates, activists (as opposed to occasional participants) in new informal groups may comprise 5%–10% of the population. Thus, the large number of such groups (some estimates put it at sixty thousand in 1989) may not necessarily convey an accurate picture of the extent of mass activism, especially since such groups form and disband rapidly in a transition period. And since activists often belong to more than one new group, simply focusing on the number of groups involves a good deal of double counting of individual participants.

Interviews with "new" activists also suggest that most had indeed been politically active before the Gorbachev era either as conventional activists or, in some cases, as dissidents. For dissidents, liberalization provides new legitimacy (or at least more tolerance). For conventional activists, liberalization provides opportunities to achieve a better fit between individual value preferences and group membership. Democratization has shifted the initiative from state to society and redefined the political space in which citizens participate. But it may also involve important continuities in political specialization if individuals with political experience and skills developed in the old system comprise a disproportionate share of the new activists.

In conclusion, our findings emphasize the need to reappraise the model of citizen involvement in the post-Stalin era in order to have an accurate basis for evaluating recent changes. We should not assume that attitudes and behavior only began to converge under Gorbachev. If we are to rewrite the book on political participation in the Soviet Union to reflect transformation in the Gorbachev era, we need to start by rewriting the chapters on participation in the past.

Appendix: Factor Analysis of Participation

In the factor analysis we used several methods of estimation, all yielding similar results. We ran the factor analysis first using all the activities in Tables 2–4, including trade union membership, and then a second time without simple union membership (on the grounds that it is all but obligatory). The results are virtually the same in both cases. That suggests that the intensity of involvement in union activities is much like the intensity of involvement in other public organizations. We opted to keep union membership in the factor analysis, because we did not want to lose information and because, for some people at least, time spent on activism in the union might preempt participation in other types of activity.

Komsomol membership is excluded because it is a very age-specific form of participation that comparatively few of the respondents could have engaged in during their LNP. Including it in the factor analysis yields a factor structure identical to that shown in Table 5. Komsomol membership loads most heavily on the third dimension, social activism, but its loading is *negative;* that is, being in the Komsomol is negatively related to being on a parents' committee or housing commission. We think this is a result of the age-specificity of Komsomol activity.

Participation in strikes was also excluded from the final analysis. When included, it loaded most heavily on the particularized contacting factor, probably because it was a form of expressing economic grievances to officials, as was the overwhelming majority of reported citizen-initiated contacts with officials and the mass media. Strikes are excluded here, however, to reduce possible ambiguity in interpreting the particularized contacting dimension. This exclusion does not appreciably affect the overall factor analytic results. When we replicated the analysis in Table 6 using factor scores that included the strike variable, the substantive results were the same as those reported above.

We used two different methods of factor analysis: principal components (PC) factoring and principal axis (PA) factoring. The results in both cases yield the same four factors, but in the PA method contacting and social activism have eigenvalues below 1.0 (the commonly accepted norm for determining whether a factor is in fact a distinct dimension). However, the factor scores from a PA solution still turn out to be highly correlated with the scores derived from a PC model: for conventional political activism the Pearson correlation coefficient between PA- and PC-derived factor scores is .936, for unconventional activism .989, for contacting .782, and for social activism .845. We chose PC because it is the preferred method for generating valid factor scores, which we wanted to use in the analysis (Dillon and Goldstein 1984, 50–51). Factor scores were derived using the regression method.

We should note that the delineation of factors could, of course, be different with a different list of activities. We are not claiming that we have exhausted all the possible types of activism; there may be other distinctions one could make. We want to emphasize only that political activities, especially conventional ones, should not be treated as interchangeable.

Notes

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1. Soviet and Western scholars have also disagreed about how much the level of participation has changed over time. Soviet evidence suggests that official data have often been exaggerated to demonstrate the success of participatory campaigns under Stalin, Khrushchev, and Brezhnev. Volkov (1965) notes, for example, that a check of activism in standing committees of local soviets in Sverdlovsk in 1954 revealed that the majority were "paper activists." See also Varchuk and Razin 1967 and Hough 1976.

2. In a 1981 survey Soviet researchers also outlined a variety of "antistimulants" to public activism: lack of interest, perceptions of activism as careerism, perceptions of the ineffectiveness of participation, lack of encouragement to become involved, and so on (Plotkin and Uueküla 1985).

3. Cheremnykh (1965) confirmed this pattern indirectly with evidence on different rates of party saturation in different conventional organizations. The highest proportions of party members were found in *ispolkomy* (executive committees of soviets), deputies' posts, and party control; the lowest in housing commissions, women's soviets, pensioners' groups, and municipal councils. See also Kukonkov 1986.

4. In a 1987 survey conducted by the Academy of Social Sciences 9% of respondents reported that they had a "real possibility" to influence government decisions (*Politicheskoe soznanie* 1988).

5. The classic discussions of the effects of ego strength and personal effectiveness on participation can be found in Lane 1959 and Campbell et al. 1960. For a review of the literature, see Abramson 1983. For an analysis of the distinction between external and internal efficacy in the literature on U.S. politics, see Balch 1976. For a discussion of political efficacy in the Soviet context, see Oliver 1968.

6. Soviet research conducted between 1968 and 1981 showed that many survey respondents were in fact less than satisfied with *obshchestvennaia rabota:* it did not bring the desired results, administrators did not adopt the recommendations put forward by participants, and the demands on time and resources were too high for participants to be very effective (Baturin 1984, 131). And Voz'mitel' (1987, 41) observed that as many as 50% of participants in *obshchestvennaia rabota* in some parts of the country wanted to participate less.

7. The data are available from the Inter-University Consortium for Political and Social Research (Ann Arbor, Michigan).

8. Participation in unsanctioned activity is significantly higher among the non-Jews in the sample because political dissidence was often their ticket out of the USSR. However, this difference proved to have little effect on the results we present below. The impact of other individual background characteristics and of individual attitudes on political activism turns out to be the same whether the respondent was Jewish or not. See the notes to Table 6.

9. Nine activists were interviewed about their previous and current political activities. Questions focused on the size of the groups they belonged to, the proportion of activists among total members, the platforms and programs the groups supported, their organizational structures, and their relations with the authorities.

10. Respondents were not asked directly whether they had belonged to the Communist party, because of the extreme political sensitivity of a question about party membership for Soviet emigrants to the United States. Pretests of the questionnaire showed that the question was too sensitive to yield accurate results. However, several questions in the survey offer an indirect measure of party membership, as we discuss in note 23.

11. It is possible, of course, that some people who were only nominal members of organizations might not even mention that they had belonged. But this is unlikely. People did mention nominal membership, especially in the organization where we would most expect it to occur (the unions). Moreover, the percentages of people belonging to any given organization and the percentages of people reporting that they took part in *obshchestvennaia rabota* are similar to the figures reported by Soviet scholars.

12. The survey also included a question on attendance at unofficial cultural events, but we omitted the item from our analysis because some SIP interviewers noted that many respondents heard the word *unofficial* as referring only to *art exhibits* rather than to *poetry readings* and *other cultural events* as the question had been intended. The interviewers did not express the same concern about other questions concerning unofficial or unsanctioned activity.

13. In the factor analysis we use all of the forms of participation included in Table 2–4 except for Komsomol activity and strikes. See Appendix for a description of the factor analysis. An earlier analysis yielded similar results (Bahry 1987), with separate factors for conventional political activism, unconventional participation, and contacting. That factor analysis, however, also included questions on both self-rated interest in politics and attentiveness to Western media, both of which we decided were more properly treated here as independent variables.

14. For the most part, the results presented in Table 6 are consistent with findings we presented earlier on Soviet citizen participation (Bahry 1987; Bahry and Silver 1987). However, we should emphasize that our measures of activism are different here; and this produces some differences in individual coefficients. The most important difference is that our measures here exclude participation in the Komsomol. As a result the level of activism among the youngest respondents is understated. We deal with Komsomol participation below. The earlier findings (Bahry 1987) also found a somewhat weaker connection between support for regime norms and conventional and unconventional participation. Those results included only respondents who answered every question about regime norms (generally respondents who were younger and more highly educated). As a result, the correlations between political activism and support for such norms were attenuated. Here, we include respondents who answered at least one of the three questions on state controls over the economy and one of the three items on collective controls over individuals.

15. Overall, the measures of objective status did better in accounting for variance in activism than did the measures of subjective status; but the variance accounted for by the subjective variables taken together (i.e., the increment to the R^2) was always statistically significant, even after the respondent's objective status was taken into account.

16. The coefficient for faith in people *is* significant at $p \leqslant .15$, but this is below the level typically accepted in social science research.

17. Pankratova and Iankova (1978, 25) suggest, for example, that one reason women become involved in what we label social activism is that it helps preserve their feeling of belonging to a larger community.

18. Shibalis (1987, 234–35) reports, for example, that one of the most common motives people named for participating in public organizations (*obshchestvennaia rabota*) was to strengthen their sense of importance or authority. In their words, participation "provides the opportunity to feel important" or to "strengthen one's authority in the eyes of coworkers and superiors."

19. Some activists, of course, played a leading role in more than one organization; thus, the data in Table 7 may include some double counting. But if we restrict our analysis to those people active in only one group and thus eliminate the double counting, the results prove to be very similar.

20. Since the dependent variable, voting, could be treated as dichotomous, we also employed a logit model in which respondents were coded as a one if they voted always or sometimes in the LNP and zero if they did not vote in the LNP. The logit model yields the same basic results as in Table 8.

21. This is confirmed by logit regression analyses (not shown) in which the dependent variables are, alternately, variables measuring whether or not the respondent had been a member of the Komsomol and whether or not the respondent had been a Komsomol leader.

22. Komsomol membership often seems to have

been a formality. According to Volkov (1965) and Kukonkov (1986), almost half of Komsomol members in several surveys spent little or no time on public assignments. However, among older generations, having been active in the Komsomol as a youth does predict subsequent conventional participation. Among people who were age 33 or older at the end of their LNP, those who had been Komsomol leaders in their youth were more likely to be compliant political activists later in life, independent of their job or social status.

23. The analysis is not shown here. The proxy measure for party membership is based on the respondents' participation in several activities known to be associated with party membership: holding a party *nomenklatura* position, working in an election campaign, belonging to the Komsomol, serving on a commission attached to a party agency or a soviet, and belonging to the peoples' militia (*druzhina*) or a comrades' court (*tovarishcheskii sud*). For further discussion, see Bahry and Silver 1990.

24. For the wording of the questions on support for regime values, see the notes to Table 6. For further discussion of these questions, see Silver 1987.

25. Similarly, leaders in such organizations tended to be somewhat more positive about the personal qualities of party officials, but they were still critical of the officials' honesty and competence.

26. DiFranceisco and Gitelman measure rates of individual participation with an additive scale based on eight activities, five of which involve the contacting of officials: writing a letter to the editor of a Soviet newspaper, contacting an *ispolkom/raiispolkom* [sic] official, contacting a procurator, contacting a People's Control Commission, contacting an elected soviet deputy, holding an elective office, and membership in the Communist party and the Komsomol.

27. The gladiator-spectator analogy was borrowed from Milbrath 1965.

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THE TINPOT AND THE TOTALITARIAN: AN ECONOMIC THEORY OF DICTATORSHIP

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I use basic tools of economic theory to construct a simple model of the behavior of dictatorships. Two extreme cases are considered: a "tin-pot" dictatorship, in which the dictator wishes only to minimize the costs of remaining in power in order to collect the fruits of office (palaces, Mercedes-Benzes, Swiss bank accounts), and a "totalitarian" dictatorship, whose leader maximizes power over the population. I show that the two differ in their responses to economic change. For example, a decline in economic performance will lead a tin-pot regime to increase its repression of the population, whereas it will lead a totalitarian government to reduce repression. The model also shows why military dictatorships (a subspecies of tin-pots) tend to be short-lived and often voluntarily hand power over to a civilian regime; explains numerous features of totalitarian regimes; and suggests what policies will enable democratic regimes to deal with dictatorships effectively.

What attitude should citizens in democratic countries adopt toward dictatorships? What policies should their governments follow? Although there is no shortage of thoughtful answers to these questions in the literature, analysis is seldom based on an explicit model capable of explaining the behavior of dictatorial governments. For example, perhaps the most provocative thesis is that of Jeane Kirkpatrick (1982), who distinguished two kinds of dictatorships. The first type, commonly referred to as "totalitarian" dictatorship, is characterized by massive government intervention into the economic and social lives of the citizenry, motivated by utopian goals of one kind or another and exemplified by communist dictatorships, Nazi Germany, and possibly contemporary Iran. The second type is what Kirkpatrick, following Friedrich and Brzezinski (1965) calls "traditional autocracies" and I call "tin-pot" dictatorships, in which the ruling government does not disturb the traditional way of life of the people and represses them only to the modest extent necessary to stay in office and collect the fruits of monopolizing political power (Mercedes-Benzes, palaces, Swiss bank accounts, etc.). Examples of this latter type include the ex-rulers Samosa of Nicaragua, the shah of Iran, Ferdinand Marcos, and General Noriega.

Kirkpatrick argued that U.S. foreign policy should be relatively tolerant toward the latter type of dictatorship. These regimes do not display much that is appealing to lovers of freedom. But although these leaders are dictators, they tend to be pro-Western and their regimes both less repressive and less stable than totalitarian regimes; and the United States, by taking a hostile attitude toward the repression of civil liberties in tin-pot regimes, often abets their downfalls only to see them succeeded by regimes of the totalitarian type. Hence, she argued, a "double standard" could be recommended to lovers of freedom in their attitude toward dictatorship; and she fought for such a standard as U.S. ambassador to the United Nations.

Now, all policy conclusions depend on a model; so it is reasonable to ask what Kirkpatrick's underlying model of dictatorship is. I think it is fair to say that in Kirkpatrick's view the essential tool or instrument of governments in either traditional or totalitarian dictatorship is *repression*, or coercion of the citizenry. Consequently, the only difference between traditional and totalitarian dictatorships is in their *level* of repression. This way of looking at dictatorship is, I believe, common to much of political science.

Perhaps surprisingly, there has been very little attempt to ask the simple question, How much of the *behavior* of dictatorships can be explained with this type of model? For example, although all dictatorships are repressive, the *level* of repression does not appear to be constant under any particular regime, whether totalitarian or tin-pot. In the USSR and Eastern Europe, for example, there appear from time to time periods of permissiveness, or "thaws," such as occurred in the Soviet Union under Khrushchev and currently under Gorbachev. In Latin America there was a resurgence of authoritarianism in the 1970s, explanations for which now constitute a fairly large literature (see, e.g., Collier 1979). In the 1980s this trend appears to have been reversed, and a period of democratization has ensued. How can these changes in the level of repression be explained?

A second question concerns the behavior of *military* dictatorships (a very common form in the modern world). If coercion or repression is the basic tool of dictatorial rule, one would expect military governments to have a comparative advantage, so that military dictatorships would be among the most "successful" in the sense that they would be stable and long-lived. Instead, we find precisely the opposite: military regimes tend to be relatively short and unstable and (what could be more ignominious) often hand power over to a civilian government after less than five years in power (Nordlinger 1977, 139; Paldam 1987). What explains the instability of military regimes, and why do military governments sometimes voluntarily cede power to civilians?

Third, consider totalitarian regimes. Although we have very little direct evidence about the popularity of these regimes in the form of opinion polls or electoral results, we do have some information about them. For example, we know that *after* the end of World War II a large proportion of the German population still supported Hitler (see the results of various polls in Kater 1988 and Larsen, Hagtvet, and Myklebusc 1980). It is also well known to scholars of comparative economic systems that the economic record of communist regimes—at least as measured by economic growth rates over long periods of time—is remarkably good (Ofer 1987). The traditional explanation of the periods of apparent popularity or dynamism of totalitarian regimes is that the people in those societies are brainwashed by the government. Repression is carried out not only against actions but against thought. Kirkpatrick's definition of totalitarian government is "one prepared to use governments' coercive power to transform economic and social relations, beliefs, values and psychological predispositions" (Kirkpatrick 1982, 101). A "totalist ideology" is the first of Friedrich and Brzezinski's six characteristics of totalitarian government (1965). Gordon Tullock develops an evolutionary argument to suggest that human beings are "readily indoctrinable" (1987, 94) and will accept any regime that has been in office for some time (although he does not accord this factor much importance in explaining the survival of dictatorship).

One obvious problem with these explanations, viewed from a contemporary standpoint, is that if totalitarian propaganda was successful in the past, what explains its dramatic failure in 1989 in the Soviet Union and Eastern Europe? But the more general problem with ideology as an explanatory variable is simply that we do not understand it very well. Thus, Friedrich and Brzezinski, after giving ideology pride of place in their explanation of the workings of totalitarianism, also suggest that the population of these societies have a profound distrust of what they are told. But if the ideology is not believed, what function does it serve? Kirkpatrick, having laid even greater emphasis on the importance of ideological indoctrination in explaining the survival of totalitarian government, brings herself at last to the central question: "But have they managed to reform human consciousness? Have they managed to educate Soviet citizens so that they would freely choose to behave according to the norms of Soviet culture *if the constraints of coercion were removed*? The answer of course is that we do not know" (1982, 123). After the events of 1989 in the Soviet Union and especially in Eastern Europe it appears that we do know the answer to this question: *no*.

I take a different approach. I look at political life as a marketplace—even in dictatorships—in which *political exchanges* take place. Repression is an important tool in the hands of the dictator; but just as a monopolist in the economic marketplace may try to impede (block, repress) entry by alternative sources of supply for the purpose of raising profits from exchange, a political dictator does not repress for the sake of repression alone but in order to increase power. In political exchange, the dictator provides individuals or interest groups with public services or patronage in exchange for support. Building support is another way to accumulate power. The problem with this strategy is that exchange in political markets—whether democratic or dictatorial—is hampered by the fact that there are no property rights in political markets to enforce exchange. Consequently, dictators, like other politicians, must find some other mechanism to resolve the problem of cheating. I will suggest that investments in political loyalty play this role.

The Instruments of Political Power

The classic means for a dictator to accumulate and maintain political power over the citizenry is political repression. Repression includes restrictions on the rights of citizens to criticize the government, the freedom of the press, and the rights of opposition parties to campaign against the government, and the outright prohibition of groups, associations, or political parties opposed to the government. To be effective these restrictions must be accompanied by monitoring of the population and by sanctions for disobedience. The existence of a political police force and of extremely severe sanctions for expressing, and especially for organizing, opposition to the government (such as imprisonment, internment in mental hospitals, torture, and execution) are the hallmark of dictatorships of all stripes.

Although political repression may be difficult to measure conceptually, a number of indexes are available, perhaps the best known of which is Raymond Gastil's (1980) civil liberties index constructed annually for a very large number of countries since 1973. Countries are ranked subjectively from one (most free) to seven (least free) on the basis of such factors as the number of political prisoners, the degree of literacy and freedom of expression, freedom of the press, and so on. The rankings are reviewed by a distinguished panel of academics. A similar index is prepared by the *Economist,* using a much wider scale (zero to one hundred).

Economists have not made much use of these data. The basic reason is undoubtedly the absence of a good theory of the determinants of political repression. On the other hand, it is an article of faith among many economists that at least some of the important determinants of political freedom are economic. For example, there is Friedman's (1956) well-known contention that free markets and political freedom go together. Bilson (1982) attempted to test for the influence of this and other economic variables on political freedom, using the Gastil data. However, the results were not very definitive. For example, the level of government intervention did not turn out to be a significant determinant of political repression in his analysis. Neither did the growth rate of GNP, the average level of government expenditure, the ratio of exports to GNP (an index of "openness" to foreign influences), and population size. The only important explanatory variable was the level of income per capita, which was negatively correlated with political repression. However, the hypothesis that all the coefficients were equal to zero was also rejected. This and the likelihood that all the independent variables themselves are highly correlated suggest that the main obstacle to progress in this area is the lack of theory.

One approach, which has not been developed in the literature, begins with the recognition that governments—democratic or dictatorial—provide services to citizens: they build roads, hospitals, and schools and protect property. Even in dictatorships policing and defense protect the population, as well as repress it. To the extent that governments provide wanted services to their citizens, it is useful to look at political life—even in dictatorships—as a market in which political exchanges are consummated.

In political exchange the government provides goods and services to the citizenry in exchange for political support. The literature on regulation and interest groups, for example, describes one kind of political exchange. In that literature politicians supply interest groups with policies that amount to some form of favorable regulation—a subsidy, tariff, control over entry, favorable tax treatment, or the like—in exchange for political support. However, one difficulty is seldom faced in this literature: the exchanges are contractually unenforceable. Indeed, no formal contract is ever drawn up. Politicians make promises, especially at election time, and hope to obtain support in exchange for them. What prevents politicians from reneging on the exchange? Of course, they often renege. The accusation that a government has "broken its promises" is one of the most commonly heard charges in any election campaign. However, if politicians *always* broke their promises, there would be little point in accusing them of doing so in any given instance. So the question remains, Why do politicians keep their promises as often as they do? And a similar problem exists on the demand side of the political marketplace: What motivates citizens and interest groups to deliver support?

Sporadic attempts have been made to deal with this problem (Alesina 1988a; Galeotti and Breton 1986; Landes and Posner 1975; Lott 1987), but no general solution commands acceptance. On the other hand, the general problem of exchange in the absence of legal contractual enforcement has received wide attention in recent years. Important contributions include Klein and Leffler 1981, Shapiro 1983, Shapiro and Stiglitz 1984, and Telser 1980). Alesina (1988a) and Lott (1987) study political markets in a similar vein. Perhaps the most comprehensive discussion of the general framework is that of Shapiro (1983). Following Klein and Leffler (1981), he studies the problem of a firm marketing high-quality goods. Prior to purchase consumers cannot distinguish high quality from low. What prevents the firm from producing low-quality goods and selling them as if they were high quality? Shapiro shows that there are three conditions required for it to be in the firm's interest not to cheat its customers: (1) the prospect of future sales; (2) the firm's past reputation as a seller of high-quality goods, and (3) the firm's receipt of a price premium on high-quality goods, both to compensate it for its past investments in reputation and to serve as a deterrent to cheating. If the present value of the premiums received from future sales is large enough to overcome the one-time gains from cheating ("milking" its reputation), it will not be in the firm's interest to cheat. A similar result is obtained in the efficiency wage literature (e.g., Shapiro and Stiglitz 1984), the central result of which is that firms will pay their employees a wage premium (i.e., wages in excess of marginal product) to prevent workers from shirking (cheating on their implicit contract).

Are these conditions fulfilled in political markets? No general answer to this question can be given here. However, political parties are long-lived institutions with reputations based on their past performance in office, and with a substantial stake in their reputation in the future (Galeotti and Breton 1986; Weingast and Marshall 1988; and Wintrobe 1987). And it seems obvious that rational politicians have the same incentives as rational sellers in economic markets to provide citizens with some reason to believe their promises. Similarly, rational politicians need to guard against "cheating" by citizens; that is, they will search for mechanisms to guarantee that citizens or interest groups can be counted on to deliver support. In general, I suggest that the reciprocal problem of cheating is solved in political markets by investments in *political loyalty*. Loyalty is typically defined as a long-term "attachment" on the part of an individual to an organization or institution (see, e.g., Hirschman 1970, 78 and *International Encyclopedia of the Social Sciences*, s.v. "loyalty"). The attachment could be rationally motivated if the individual has some reason to believe that the organization will look after his or her interests in the future. Political parties will therefore look for ways to engender this belief. Two institutional mechanisms that engender loyalty are pork barrel projects and political patronage. These are frequently discussed in the literature on congressional politics (Ferejohn 1974; Weingast, Shepsle, and Johnsen 1981).

To illustrate, suppose that a politician obtains a dam or bridge or other pork barrel project for the citizens in his district. A proper cost-benefit analysis will reveal that the project is "wasteful." If the project is not wasteful, citizens have no reason to deliver support to that politician in exchange for the dam or bridge since the politician is making no sacrifice on their behalf but simply doing what any technocrat would do. On the other hand, to the extent that the project *is* wasteful (i.e., a genuine pork barrel project), citizens have some reason to believe that that politician or party is responsible for their getting the project and will look after their interest in the future, provided they reciprocate with support. More precisely, the element of waste in the project is precisely the sign that the politician can claim credit for the project, as suggested by Ferejohn (1974) and others. A patronage job acts in exactly the same way. So pork barrel projects and patronage jobs are the exact analog in political markets to price and wage premiums in economic markets. The size of the premium is measured by the amount of waste in the project.

It immediately follows that the stock of loyalty capital of this type may be measured by the total value of rents distributed by the political party in office.

The total costs of rent seeking (Posner 1975, Tullock 1967), which have been frequently measured in recent years, may be waste in the economic marketplace; but in the political marketplace these costs are useful investments that reduce the transaction costs of political exchange. The group that receives the rents immediately has some reason to believe that that political party will look after its interest in the future. Since the rents can be withdrawn, politicians have reason to believe that the group that receives them will provide the politicians with loyal support. The rents act as a premium to compensate the interest group for its support or loyalty to the party and serve as a deterrent to the shifting of loyalty. Consequently, pork barrel-projects and political patronage are investments that solve the problem of political cheating on both sides of the market.

Of course, there are many other such institutions and many other ways in which politicians try to get citizens to believe in them and support them. (Lott [1987], for example, argues that ideology is a precommitment device.) Political markets are subtle and complex. The central point here is that rational politicians have an incentive to discover these and invest in them.[1] We will call the total stock of such investments politicians' *loyalty capital.* These stocks are measurable. (Galeotti and Forcina [1989], for example, measure "loyalty ratios" for U.S. political parties.)

Political dictators have the same incentive to build loyal bases of support as democratic politicians. In this respect the exchanges between dictators and interest groups are no different from those between democratic politicians and interest groups, as has been argued, for example, by Becker (1983). Willerton (1987) identifies a number of patronage links in the Soviet system under Brezhnev. Dictators are also often interested in building popular support, which I will argue is a characteristic of stable, or long-lived, dictatorships. That both Hitler and Mussolini amassed considerable popular support is well known (see, e.g., the studies in Larsen, Hagtvet, and Myklebusc 1980) and is the basis for the claim by Lipset (1960) and others that the masses are a threat to democracy when politically active.

The Model of a Tin-Pot Regime

The Equilibrium Levels of Loyalty and Regression

I have argued that a dictator can use two instruments to build and maintain political power over the population under the government. The first instrument involves *political repression,* that is, outlawing opposition to policies. The use of this instrument requires resources to produce repressive legislation, publicize these laws, police their obedience, and punish offenders. Alternatively, the dictator can bind parts of the population to him as loyal supporters through the creation and distribution of political rents. Rents cost resources and generate a deadweight loss to the economy (of course, expenditures on repression are also deadweight losses). The dictator who wishes to remain in office therefore faces a trade-off between these two alternatives. The interrelationships between them, however, are complex. The main complication is that while loyalty and repression both use up resources (and in that sense are alternative "inputs" into the creation and maintenance of political power), their levels are not independent of one another: the level of repression affects the supply of loyalty. In order to sort out the various relationships involved, I now construct a simple model of the equilibrium levels of repression and political loyalty.

First, I assume that the relationship between the inputs of loyalty and repression and their output (power) can be represented by the production function

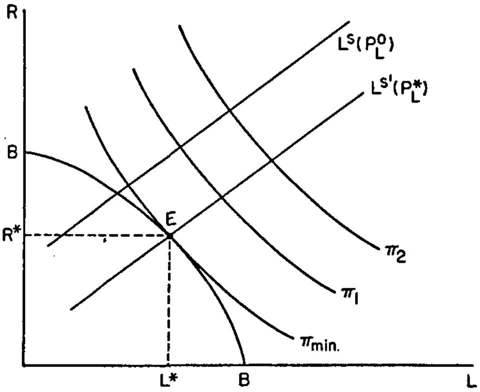
$$\pi = \pi(L, R). \qquad (1)$$

The production function for power (Π) is assumed to be "well behaved," that is, $\pi_L, \pi_R > 0$, $\pi_{LR} > 0$, $\pi_{LL}, \pi_{RR} < 0$. These relationships imply in the production of power diminishing returns to the continued use of either instrument alone and that there is some complementarity between repression and loyalty in the production of political power. Figure 1 depicts this production function in the form of a set of isopowerlines, where, as is usual, higher isopowerlines denote higher power.

Second, I assume that both the amount of loyalty available to the dictator and its price are fixed in the short run but variable in the long run. On the other hand, the level of repression is variable in the short, as well as in the long run.

Third, I distinguish two kinds of dictators: totalitarians and tin-pots. Totalitarian dictators maximize power over the population under their control. I believe this objective function is consistent with the characterization of totalitarianism in Arendt (1951), Hayek (1944) and Friedrich and Brzezinski (1965).[2] The objective function of a tin-pot dictator, on the other hand, is to miminize the resource cost of staying in office. In Figure 1, the tin-pot dictator seeks no more power over the population than represented by the lowest isopowerline in the figure, π_{min}. At any lower level of power, the tin-pot will be deposed. The tin-pot who obtains more resources than required to attain $\pi = \pi_{\text{min}}$ (resource constraints will be discussed shortly), does not spend them on repression or loyalty but on personal or family consumption. Since the tin-pot always remains on π_{min} (as long as he or she stays in office), it immediately follows that there is an inverse relationship between the amounts of L and R demanded by the tin-pot: an increase in R results in a fall in the level of L demanded.

Figure 1. Equilibrium Levels of Repression and Loyalty Under a Tin-pot Regime



Consider now the supply of loyalty to a tin-pot dictator. I assume that the tin-pot, although having a monopoly in terms of formal political office, does not monopolize political power in the country but faces opposition in the sense of potential alternatives to the government. Citizens and interest groups may establish (possibly covert) ties with these potential opposition leaders. In other words, the dictator has a monopoly of formal (legal) political power but no monopoly on political loyalty. What happens to the supply of loyalty to the tin-pot if the level of political repression is increased? To analyze a typical citizens' response, recall that in our framework loyalty to the government or to opposition leaders is a capital asset accumulated to facilitate political exchanges. Each citizen may be viewed as accumulating an optimum "portfolio" of these assets, taking into account their expected rates of return and their risk. As in the standard theory of portfolio choice (see, e.g., Arrow 1971, for an exposition), a change in the riskiness or rate of return of any asset will lead the investor to change his desired portfolio; and this change may be decomposed into the usual income and substitution effects. In other words, citizens or interest groups who speak out against the government, demonstrate against it, and so on are essentially offering their loyalty to someone willing to offer an alternative policy. An increase in repression affects the risks and return to accumulating these assets.

The increase in repression may take the form of an increase in the range of activities prohibited, in the level of policing of forbidden activities, or in the size of the sanction imposed for those caught engaging in them. Whichever form it takes, the risks of disloyalty are increased, and its expected rate of return diminished. Consequently, the attractiveness of dealing with the opposition decreases, and the *relative* attractiveness to citizens and interest groups of exchanges with the dictator or the dictator's representatives *increases*. This substitution effect implies that a typical citizen's loyalty (and hence the aggregate supply of loyalty to the dictator) will be positively related to the level of repression. However, there is an income effect that works in the opposite direction: an increase in repression increases either the likelihood of a "loyal" individual's being the victim of a sanction or the size of the sanction imposed. This reduces the individual's wealth and (so long as investments in political loyalty are a normal good) reduces investments in political loyalty to the regime (as well as to the opposition). At low levels of repression this effect will be small for most individuals. For example, if, as in the early years of Nazi Germany, repression is directed mainly at obvious opponents of the regime and at Jews, the loyalty of these groups would obviously be reduced; but persons who fell into neither of these categories could reasonably assume that they would not be victims of the regime's repressive policies. Consequently, so long as the level of repression is relatively low, it seems reasonable to assume that the substitution effect dominates the income effect for most citizens and therefore that the aggregate supply of political loyalty is positively related to the level of repression, as depicted by the L^S curves in Figure 1.

The supply of loyalty to the dictator will also depend on other variables apart from the level of repression. In our framework citizens and interest groups supply loyalty because they expect to receive some portion of the gains from political exchange in return. This return or rent to suppliers can be represented as a "price" received per unit of loyalty supplied (P_L^S). The distinction between the price received and the price paid will be discussed shortly. I assume the supply of loyalty to be positively related to this price. In Figure 1, an increase in P_L^S from P_L^0 to P_L^* therefore results in a rightward shift of the supply curve, as depicted.

Finally, the supply of loyalty also depends on the economic performance (*PE*) of the regime. For example, suppose that performance were better than expected. Then the value of a given fraction of the rents from political exchange will tend to rise, and the supply of loyalty will increase.

For these reasons, the supply of loyalty function can be written as

$$L^S = L^S(R, P_L^S, PE), \tag{2}$$

where $\partial L^S/\partial R > 0$, $\partial L^S/\partial P_L^S > 0$, $\partial L^S/\partial PE > 0$.

The model can be completed by formally introducing the resource costs to the dictator of repression and loyalty. The costs per unit of repression, P_R, are the costs of obtaining manpower and capital equipment for the police, prisons, and court system. It seems reasonable to assume that these per unit costs are not under the tin-pot's control, although some dictators may be able to "produce" repression more efficiently than others, as will be discussed shortly.

The cost to the dictator of creating and maintaining loyalty (P_L^D) include the costs of creating and distributing monopoly rents, building dams and bridges, doing favors for the citizenry, and so on. The creation of loyalty is a rather subtle process, since it essentially involves the creation of a belief on the part of the citizenry that the dictator will look after their interests. The costs involved therefore include the costs of signaling and communicating in addition to the literal costs of such pork barrel projects as dams and bridges. It follows that the price paid for a unit of loyalty capital by the dictator (P_L^D) differs from the price received by suppliers of loyalty (P_L^S) because the former includes all of the costs incurred by the dictator to create and maintain loyalty while the latter includes only the portion actually received by suppliers of loyalty. So, for example, an increase in the efficiency of producing loyalty implies a lower P_L^D but not necessarily a lower P_L^S, although normally the two prices will move together. Apart from such exogenous changes, we shall assume that the ratio P_L^D/P_L^S is fixed. In what follows, to simplify the notation, the superscripts *S* or *D* will be dropped whenever they are not required.

In order to represent the dictator's expenditure function, it is convenient to rewrite equation 2 with P_L instead of *L* as the dependent variable:

$$P_L = P_L(L, R, P\bar{E}). \tag{3}$$

in which case $\partial P_L/\partial L > 0$, $\partial P_L/\partial R < 0$, $\partial P_L/\partial PE < 0$. The dictator's expenditure function is then simply

$$B = \bar{P}_R R + P_L(L, R, P\bar{P})L \tag{4}$$

Equation 4, which incorporates the supply-of-loyalty relationship of equation 3, is depicted by the concave "budget" line *BB* in Figure 1. *B* does not represent the total budget of the government, since it does not include resources spent for the dictator's personal consumption. Rather, *B* represents the resource costs to the dictator of staying in office. The dictator is not constrained by any particular level of *B*, since the dictator has the powers (at least to some extent) to increase such resources through increased taxation, borrowing, or simple confiscation (though these may of course have deleterious effects on the economy and therefore on the dictator's future consumption prospects). A simple way of representing the dictator's *total* budget is discussed shortly. For the moment, we simply note that resources spent on *R* or *L* to maintain hold on power are thereby diverted from the tin-pot's personal consumption. For this reason the dictator will (in the long run at least) wish to minimize the resource costs of staying in office and consequently be sensitive to P_L/P_R, the relative per unit costs of *R* and *L*.

The tin-pot's long-run problem is to minimize the resource costs of staying in office, that is, minimize equation 4 subject to equation 1:

$$\begin{aligned}\text{Min } V = {} & P_R R + P_L(L, R, \bar{P}\bar{E})L \\ & + \lambda[\Pi_{\min} - \Pi(L, R)]\end{aligned}$$

with respect to the choice of *R* and *L*. (*PE* is assumed exogenous for the time being.)

This yields

$$\frac{\partial V}{\partial L} = \frac{\partial P_L}{\partial L}L + P_L - \lambda\frac{\partial \Pi}{\partial L} = 0 \tag{5}$$

$$\frac{\partial V}{\partial R} = \frac{\partial P_L}{\partial R}L + \bar{P}_R - \lambda\frac{\partial \Pi}{\partial R} = 0, \tag{6}$$

which may be combined to obtain the familiar-looking expression

$$\frac{\Pi_L}{\Pi_R} = \frac{P_L + L\dfrac{\partial P_L}{\partial L}}{P_R + L\dfrac{\partial P_L}{\partial R}}, \tag{7}$$

where $\Pi_L = \partial\Pi/\partial L$, $\Pi_R = \partial\Pi/\partial R$. Equation 6 resembles the usual condition for cost minimization except that the marginal cost of repression $P_R + L(\partial P_L/\partial L)$ is less than its price because an increase in R allows the dictator to obtain loyalty at a reduced price ($\partial P_L/\partial R < 0$).

The long-run equilibrium described by equation 6 is represented by point E in Figure 1 and is unique if the production function is well behaved and the budget line concave. The upward-sloping supply-of-loyalty function does not pose an additional constraint (i.e., it *must* pass through point E), since this relationship is incorporated into the budget constraint. Its main usefulness will be apparent when we turn to comparative statics.

An interesting special case of this general model may be described if we further assume that the tin-pot's *total* budget (i.e., including government resources diverted to personal use, whether for personal consumption or deposited in Swiss bank accounts) arises solely from a proportional revenue-maximizing income tax. As in Brennan and Buchanan (1980), let t = tax rate and y_0 = initial (pretax) income. The tax reduces work effort, and therefore income, by η (the elasticity of income with respect to the tax rate). Tax revenue is then $ty_0(1 - \eta t)$. Assume also that the rate of growth of disposable income of the population ($\dot{Y}_D$) is a good proxy for PE, the dictator's economic performance.[3] Then the tin-pot may be described as maximizing "profits," that is, the difference between total revenue and the total costs of staying in office, as in

$$\begin{aligned} \text{Max } Z = {} & tY_0(1 - \eta t) - P_R R \\ & - P_L(R, L, Y_D)L \\ & + \lambda[\Pi_0 - \Pi(L, R)], \end{aligned} \tag{8}$$

where $\dot{Y}_D \equiv$ the rate of growth of $(1 - t)Y_0(1 - \eta t)$. Maximizing equation 8 with respect to t, R, and L yields the same first-order conditions for R and L described in equations 5 and 6 (or 7) and yields the further condition for t,

$$\frac{\partial Z}{\partial t} = Y_0(1 - 2\eta t) - \frac{\partial P_L}{\partial t} L = 0$$

or

$$Y_0(1 - 2\eta t) = \frac{\partial P_L}{\partial t} L, \tag{9}$$

where $\partial P_L/\partial t \equiv (\partial P_L/\partial \dot{Y}_D)\ (\partial \dot{Y}_D/\partial t) > 0$.

The left-hand side is the marginal revenue from an increase in the tax rate; and the right-hand side is the marginal cost, which arises because higher taxes reduce political support or loyalty and must be compensated for by paying a higher price for loyalty to keep L from falling and reducing Π below Π_0.

From equation 9, the optimum tax rate is

$$t^* = \frac{1}{2\eta}\left(1 - \frac{\frac{\partial P_L}{\partial t} L}{Y_0}\right)$$

This differs from Brennan and Buchanan's revenue-maximizing tax rate, which is simply $t^* = (1/2)\eta$. The "profit-maximizing" tax rate described in equation 9 is lower than this, since $\partial P_L/\partial t > 0$. The present analysis is perhaps more general, since it incorporates the concern facing any dictator that raising tax rates increases opposition to the regime. No such problem arises for Brennan and Buchanan's "Leviathan," who is assumed to have no difficulty in staying in office even at confiscatory tax rates.

Effects of Economic Performance on the Behavior of a Tin-pot

To illustrate the workings of the model, suppose first that the economic performance of the regime improves and that the regime is able to claim the credit for this. The analysis is straightforward, as a simple graphic proof shows. In Figure 2, E_O

depicts the initial equilibrium of the system, at which R, L, and P_L are given by R_0, L_0 and $P^0{}_L$. The improvement in economic performance shifts the supply of loyalty to the right to $L^{S'}$. In the short run there is now an excess supply of loyalty ($L_1 - L_0$). At L_1, R_0 of repression is unnecessary to remain in power; and the dictator will relax R to R_1, implying a new short-run equilibrium at E_1. In the long run, however, the excess supply of loyalty implies that the dictator can allow the price paid for it, P_L, to fall. This shifts L^S back partially to $L^{S''}$ and also changes the slope of the budget line (to BB_1). Since (P_L/P_R) falls, the final long-run equilibrium must be at a point to the right of E_0, for example, at E_2 in the figure on B_2B_2 (parallel to BB_1), implying an increase in L (to L_2) and a fall in R (to R_2). In other words, under a tin-pot dictatorship, an improvement in economic performance unambiguously results in an increase in loyalty and a fall in the level of political repression.

Note that if strong economic performance were to continue over a period of time, the level of repression would continue to fall; and ultimately it is possible that the tin-pot would need to use very little repression to stay in office and could even hold and win a reasonably free election.

Suppose, on the other hand, that there is a deterioration of economic performance. The supply of loyalty shifts to the left (from L^S to $L^{S'}$), as shown in Figure 3. At R_0 the supply of loyalty has fallen to L_1; and R_0 of repression and L_1 of loyalty are insufficient to keep the dictator in office (i.e., $\pi < \pi_{\text{min}}$). The proper short-run response by the dictator is to increase R to at least R_1, implying a new short-run equilibrium at E_1. Thus, the short-run response to changes in economic performance is asymmetric: a deterioration in performance calls for a sharp rise in repression, while an improvement results only in a slow relaxation of much smaller magnitude (compare $R_0 - R_1$ in Figure 3 to $R_0 - R_1$ in Figure 2). The asymmetry arises for two reasons: (1) the increase in repression when performance declines is necessary for survival (in office), whereas only greed motivates a fall in R when performance improves; (2) the diminishing mar-

Figure 2. Improvement in Economic Performance Lowers Repression Under a Tin-pot Regime

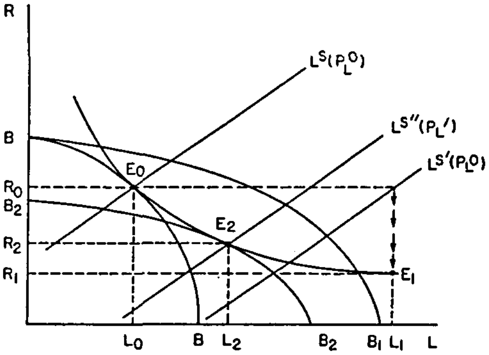
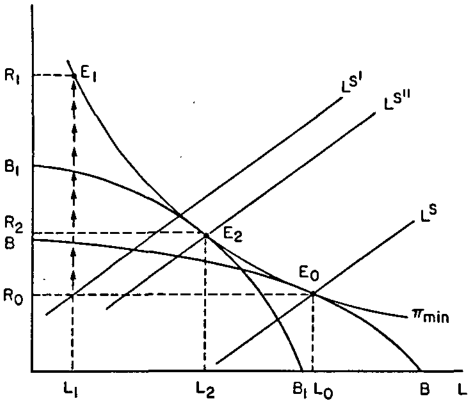


Figure 3. Decline in Economic Performance Raises Repression Under a Tin-pot Regime



Note: The short-run increase in repression in Figure 3 (R_1–R_0) is larger than the decrease (R_0–R_1) in Figure 2.

ginal rate of substitution between R and L implies that the absolute size of the increase in R required to stay in office when performance declines is larger than the decrease possible when performance improves. Because of this asymmetry, a cross-sectional regression between some index of economic performance (such as the growth rate of national income used in Bilson [1982]) and a repression index will not necessarily reveal the true relationship between these two variables.

Of course, dictators, especially tin-pots, are not perfect and sometimes make mistakes. If they did not, then provided they had command over sufficient resources to obtain $\pi_{\min}$, they would never be deposed. The classic error for a tin-pot dictator is to respond to worsening economic conditions by relaxing repression rather than increasing it. (For reasons that will become apparent later, I call this the error of a tin-pot who confuses himself with a totalitarian.) For example, there is evidence that the shah of Iran responded in this fashion to the unrest resulting in part from the considerable deterioration in economic conditions after 1976, and the result was his ouster (Arjomand 1986).

In the long run, maintaining repression at R_1 is both expensive (since R_1, L_1 is not the cost-minimizing solution) and unnecessary (since at R_1 the supply of loyalty will expand beyond L_1). Consequently, in the long run, the dictator can allow R to drift down, provided that P_L is increased to eliminate the excess demand for L that would arise again if R were to fall back to R_0. The rise in P_L shifts L^S partially back to $L^{S''}$ and twists the budget constraint in the direction shown in Figure 3 so that the new long-run equilibrium is at E_2, implying (compared to the initial equilibrium) a fall in L and increase in R. In other words, a deterioration in the economic performance of a tin-pot regime results in a fall in loyalty and an increase in political repression.

Military Regimes

A number of different types of military regimes can be distinguished, depending on the degree of civilian power sharing. Here we follow Nordlinger (1977, 3) and consider a military regime to be one where power was obtained through a coup, where the highest governmental officials have served (or continue to serve) in the armed forces and the governors are primarily dependent on the support of the officer corps for the retention of power.

Two issues are of particular interest concerning military regimes: Why are they so unstable? and Why do they often voluntarily hand power over to civilian governments? To begin the analysis, assume that compared to a civilian regime, a military regime has a comparative advantage at repression and a comparative disadvantage at accumulating political loyalty. The assumption that the military would have an advantage at repression is easy to understand. After all, the central skill of the military is, in Lasswell's well-known phrase, "the management of violence." However, in addition, it has often been argued that the military are disadvantaged compared with civilian regimes at building a civilian political base. Thus, Stepan suggests that "a military government does not easily tolerate a normal level of dissension and debate needed to build or maintain coalitions with civilians" (1971, 263). Perlmutter and Bennett note that historically, military regimes have shown "an inability to tolerate political participation" (1980, 20) and suggest that the reason for this is that the mass participation required to do this is thought to be closely related to political turbulence and the possibility of violence. Thus, although no quantitative evidence has, to my knowledge, been assembled on this issue, the general consensus among observers of these regimes seems to be, summed up by Eric Nordlinger: "Few [military regimes] have attempted to build mass parties and where they have been created they turned out to be ineffectual structures because genuine participation was not permitted" (1977, 58).

Assume, then, that the resource constraint for the military is steeper than that for a civilian regime (e.g., *BB* compares to B_2B_2 in Figure 2). The initial equilibrium for the military regime is at a point like E_0 in Figure 2 (compared to E_2 for the civilian regime), implying the use of more repression, *R*, and less political loyalty, *L*, compared to the civilian regime. What makes this military regime unstable? To answer this question one has to probe into the motives behind the intervention of the military into political life. While these are undoubtedly many and complex, ranging from disgust at the behavior of civilian politicians, fear of communist takeover, and the desire to restore order to political life, there is one motive that stands out for its simplicity and generality—the desire to increase the budget of the military. There is considerable evidence to support this point. Putnam (1967) classified twenty Latin American countries according to the extent of military intervention and found a positive correlation between this variable and defense spending as a percentage of gross national product. Schmitter (1971) classified Latin American countries into three groups according to the level of military intervention and found that central government expenditures devoted to the military ranged from 9% to 14% and then 19% as the level of military intervention increased. Nordlinger (1970) found that the average proportion of gross national product allocated to defense expenditures was almost twice as high in countries with a pretorian officer corps than in those where the military accepted civilian control.

In other words, one straightforward way to explain military intervention in politics is to model the military as a budget-maximizing bureau and to note that intervention is a strategy available to the military for increasing its budget in countries with weak political systems. As I argue elsewhere (Breton and Wintrobe 1975, 1982) budget maximization is too specific a maximand to satisfactorily represent the objectives of most bureaus, mainly because of the existence of substantial opportunities for mobility among the different bureaus in most bureaucracies. However, this objection does not apply to the military, which tends to be a closed hierarchy. Since career opportunities for military personnel tend to be limited in bureaus outside the military hierarchy, budget maximization is often the most attractive strategy available to raise the salaries, prestige, and power of military personnel. Consequently, the objectives of military dictators are not necessarily the same as those of tin-pot regimes generally.

Suppose, then, that the military takes power from a civilian regime. Initially, the equilibrium shifts from E_2 to E_0 in Figure 2. The military then proceeds to do what a military government does best, namely, raise the salaries of military personnel. This has a peculiar consequence, which is that it raises the price of repression, P_R; that is, it destroys the comparative advantage of the military in governing. Consequently, having achieved its primary aim (increasing the military budget), the rational thing for the military government to do is to hand power over to a civilian regime, having received suitable guarantees of immunity from both prosecution and budget cuts by the new regime. Of course, a military government that seeks to remain in office and not merely to raise the military budget will sense the danger to its capacity to govern of excessive salary increases to its personnel and may attempt to limit salary increases or to restrict them to certain subgroups in the military. This, however, tends to breed internal dissension, to destroy the internal cohesiveness of the military hierarchy, and possibly to breed a countercoup. Countercoups are quite common in military governments: according to Kennedy (1974), they were twice as likely in a military as in a civilian regime, and the vast majority involve the overthrow of one military government and its replacement by another.

Consequently, it is not difficult to explain either the instability of military regimes or why the vast majority of military regimes tend to end via voluntary withdrawal of the military from power. There is nothing peculiar about the fact that the military, once in power, tends to reward its supporters. Presumably, all political regimes do this. Political parties backed by unions presumably reward unions when in power, political parties backed by business groups presumably reward them when in power. What is peculiar about military governments is that in the process of rewarding their supporters they tend to weaken, rather than strengthen, their own capacity to govern. While this point is obviously not a complete explanation of these regimes, it does explain both the instability of military regimes and why they typically end through a voluntary transfer of power to civilian regimes.

Totalitarian Regimes

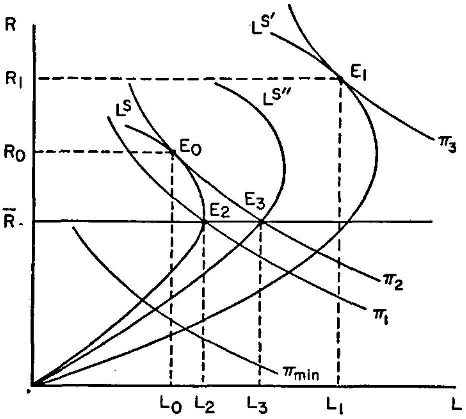
The differences between the totalitarian and tin-pot regimes are reflected in their respective maximands. In a totalitarian regime I assume that the dictator, leader (Hitler, Stalin), or great helmsman (Mao) uses the instruments of repression and loyalty to maximize power over the population under control. I believe this objective is consistent with the descriptions of such regimes in Arendt (1951), Hayek (1944), and Friedrich and Brzezinski (1965). The classic historical examples are Nazi Germany and Stalin's Russia in the 1930s. For example, at the height of Hitler's power the population of Germany was held not to be subject to any laws but exclusively to the "will" of the Fuhrer as divined from his speeches and other remarks. Subjection to laws would amount to circumscribing the power of the leader (Arendt 1951). How useful the various concepts of totalitarian rule are and how much they are applicable to other societies, especially post-Stalinist Russia and other contemporary communist societies, is a hotly debated question.[4] In any case, extreme cases are often useful analytically; and this conception of totalitarian regimes is useful in that it places them at the opposite extreme from tin-pots. Most real-world dictatorships undoubtedly lie somewhere in between.

From an economic point of view, the central question is not so much the maximand as the nature of the constraint on the totalitarian leader's maximization of power. Budget considerations may pose a constraint,[5] particularly at the point where revenue-maximizing taxes have been imposed on every available tax base. However, the leader can always nationalize or confiscate resources directly. Deleterious economic effects such as deadweight losses (which constrain the behavior of ruling interest groups according to Becker 1983) may cause problems but do not necessarily reduce the power of the leader. Supposing, then, that we ignore budgetary and price constraints for the sake of argument, is there any other constraint on the totalitarian leader's maximization of power?

From the leader's point of view, the key consideration is the loyalty of the population. The leader's problem is depicted in Figure 4. So long as the aggregate supply-of-loyalty curve is upward-sloping, the dictator can increase personal power over the population by increasing the level of repression. Consequently if the L_S curve were upward-sloping throughout its range, the only possible equilibrium would be a corner solution involving the perfect repression of the population (i.e., "the permanent domination of each single individual in each and every sphere of life" [Arendt 1951, 325]), a goal that Arendt often suggested the Nazi regime was heading to or even had attained. However, considerable evidence has subsequently been amassed indicating that this goal was not achieved by the Nazi regime (see, e.g., Broszat 1981). In addition, theoretical considerations point to a conflict between perfect repression and the maximization of power over the population.

Recall that an increase in repression induces opposing effects on the supply of loyalty to the regime. The substitution effect (the change in the amount of loyalty supplied as a result of a fall in the return, or increase in the risk, of disloyalty) always favors the regime. On the other hand, an increase in the probability of being discovered to have links to actual or potential opposition movements or an increase in the sanction imposed for this offense reduces expected wealth; and this reduction in wealth has an income effect that leads an individual to reduce all investments in political loyalty, including those with the regime. At low levels of repression it is reasonable to assume that the income effect is small for most people, so that it is dominated by the substitution effect (as argued earlier in the context of my analysis of tin-pot dictatorships). However, as the level of repression increases, the income effect gets larger. In addition, as the level of repression increases, the number and the size of groups opposed to the regime become smaller; and at very high levels of repression opposition to the regime tends to get wiped out. Consequently, the substitution effect becomes vanishingly small as the level of repression becomes very large. Ultimately, then, a point must be reached where the income effect overwhelms the substitution effect for most citizens, causing the aggregate supply of loyalty to the regime to bend backward as depicted in Figure 4.

Figure 4. Equilibrium Levels of Repression and Loyalty Under a Totalitarian Regime, and How They Are Affected by Changes in Economic Performance and Western Policy



This point is reinforced if we note that the process of repression will usually be subject to error. The real opponents of the regime do not typically identify themselves as such, and actual or potential opposition to the regime will have to be inferred from certain behavior and characteristics. At low levels of repression the identification of opposition leaders is a relatively simple task. At higher levels of repression it becomes more and more complex. Consequently, even perfectly loyal individuals will have to be more and more careful in establishing or maintaining network links: it may turn out that their investments will, after the fact, have been made with the "wrong" people. This explains the great effort made in the Stalinist purges of the Communist party of the late 1930s, to wring fabricated "confessions" from the accused in the infamous Moscow trials. The more that doubts persisted within the party as to the real guilt of the accused, the more party members could reasonably be expected to withhold making any investments—even from perfectly loyal motives—for fear that they themselves would become victims of the terror. Indeed, there is some evidence that even Stalin felt that the purges did go too far by the late 1930s (Schapiro 1971, 435–438). This explains both the subsequent "liquidation of the liquidators" and the ultimate relaxation of the terror at the end of 1938.

If the supply of loyalty becomes backward-bending, "optimality" for the totalitarian dictator is at a point like E_0 in Figure 4, where the backward-bending supply curve is tangent to the highest attainable isopower line.

Formally, he leader chooses R and L to maximize power subject to the constraint posed by the supply of loyalty; that is, the leader's problem is: max $\Pi(L, R) + \lambda[L^S(R, \overline{PE})]$. The solution is simply $\Pi_R/\Pi_L = \partial_R/\partial_L$, which shows that if the supply of loyalty is the only constraint, the slope of the supply curve must be the same as that of the isopowerline at the optimum point (E_0 in Figure 4). Note that the dictator might still be able to increase personal power by better-than-expected economic performance (PE). But at E_0 the limits of *repression* as a means of increasing power over the population have been reached.

One implication of this model is that in a totalitarian regime repression is carried to the point where at the margin an increase in repression reduces the supply of loyalty. The opposite prediction holds for a tin-pot. Consequently, one test of whether a dictatorship is totalitarian or tin-pot concerns the marginal behavior of the supply of loyalty in response to a change in the level of repression at the equilibrium point. Interestingly, some evidence on this question has recently been collected for the current Soviet regime. Using data from the Soviet Interview Project, Bahry and Silver (1987) computed a "KGB differential"—the relationship between the estimated competence of the KGB and the degree of support for various goals of the regime; that is, the subjects (2,793 Soviet émigrés) were asked to rank the leaders of various Soviet institutions on different scales, including a scale for competence. They were also asked how much they supported various regime goals. Bahry and Silver found that while support for regime goals was positively related to the estimated competence and honesty of various institutional leaders (excluding the KGB), it was (significantly) negatively related to the estimated competence of the KGB. Thus, whatever the limitations of the procedure and the data,[6] the results do indicate that at least so far as these respondents were concerned, the Soviet Union under Brezhnev was still a totalitarian regime.

A totalitarian regime also differs from a tin-pot in its response to exogenous shocks. Suppose, for example, that the performance of the regime were to improve and that the leader was able to claim credit for this. The improvement can be, as with tin-pots, in any kind of performance, including success in war or solving some major political problem; but we focus here on economic performance, such as an improvement in the rate of economic growth or a reduction in inflation or unemployment. An improvement in any of these dimensions of performance shifts the supply-of-loyalty curve to the right, as depicted in Figure 4. The new equilibrium is at E_1. The power of the regime over the population increases unambiguously (since the supply of loyalty shifts to the right, the new L_S curve, L'_S must be tangent to a higher isopowerline than is the old supply curve L_S). And for the most obvious cases (e.g., a parallel shift of the supply curve and homogeneous isopowerlines) the level of repression tends to increase as well. The reason is simple: the increase in the supply of loyalty as a consequence of the increase in economic or other performance provides the leader with an opportunity to amass more power. The leader maximizes power (unlike a tin-pot, who would simply squander the extra resources on personal consumption) and so takes this opportunity.

In the Soviet Union and the satellite communist countries of Eastern Europe economic performance for many years was surprisingly good. For the last twenty-five years or so, however, economic performance as measured by rates of growth and rates of growth of productivity has steadily deteriorated (Ofer 1987). The present model predicts that in response to this deterioration the levels of power, loyalty, and repression will all diminish. I know of no direct evidence on the behavior of these variables, but some indirect evidence supports the proposition that repression is positively related to economic performance. Stephen White (1986) argues that as economic performance in Eastern Europe has deteriorated, the parties have looked for other ways to secure "legitimacy." He notes (1) increasing use of the electoral mechanism—multiple candidacies in communist "elections" were an isolated phenomena in the 1960s but are characteristic of over half of the countries of Eastern Europe in the 1980s; (2) political incorporation—the proportion of the population who are members of the party has increased steadily from 3%–4% in the 1950s to 6%–10% in the 1970s and 1980s; (3) associational incorporation; and (4) the instigation in successive countries of the practice of allowing letters to be written to the party, the state, and the press. Events in Eastern Europe in 1989 have, of course, carried this search for legitimacy beyond imagination.

From the point of view of the present model all of these changes can be interpreted as reductions in the level of repression of the population. In the Soviet Union itself the current wave of *glasnost* may obviously be interpreted along the same lines, namely, a fall in repression in response to the deterioration in economic performance. It is also worth noting that the increase in repression in China follows a period of dramatic improvement in that country's economic performance. This is not the place to comment in detail on the breathtaking recent events in the communist bloc, but it is worth noting that the positive correlation between the level of economic performance and the level of repression is precisely what is to be expected from a totalitarian regime. Of course, where the fall in repression in the Soviet bloc is accompanied by fundamental institutional changes that alter the character of the regime (e.g., genuine multiparty elections), these events might be irreversible; and repression might not necessarily accompany an improvement in economic performance, as the model predicts would occur in the absence of the institutionalization of democratic safeguards during the period of liberalization.

Finally, and perhaps most importantly, the model implies that the *average* level of repression and the *average* level of loyalty are positively correlated in totalitarian regimes. In Figure 4 both repression and loyalty increase for movements along a given supply curve (except at the margin). This suggests that the level of loyalty to the regime in totalitarian regimes is high compared to tin-pot regimes. I summarize and interpret informal evidence that this was true for the Nazi regime in Breton and Wintrobe 1986 and for the Soviet regime in Wintrobe 1988.

Communist systems are currently in steep decline, and loyalty to them is now undoubtedly very low. I have suggested some of the reasons for this decline (Wintrobe 1988). Here it is worthwhile to recall that the system appeared to function very well at least until the end of the 1960s and to inquire how it functions when it is functioning well. A good model of totalitarianism can only be one that explains its successes and failures over the previous half century.

The interpretation of totalitarian regimes suggested by this model is quite different from those common in the literature, including both proponents and critics of the concept of totalitarianism. It is also very different from the "command economy" construct widely used in the economic literature on socialist countries. From my point of view the repression characteristic of totalitarian societies also helps to build a core of loyal supporters whose relationship with the regime is one of exchange rather than coercion. The regime accommodates these offers of support through an institutional mechanism facilitating the accumulation of loyalty and enforcing exchanges. This institution is the totalitarian party.

The Communist Party of the Soviet Union, for example, is organized on three principles: it controls production and distribution, it permeates the bureaucracy, and it is long-lived. The Party's control over the productive system holds in a formal sense (article 6 of the Soviet constitution specifies that the Party is the "leading and directing force of Soviet society") and, at least in the industrial sector, in an informal sense, since the Party permeates the productive system: every factory of even modest size must contain a Party cell. Consequently, the Party has much stronger control over the bureaucracy than the governing party in a democracy has over the civil service. The Party controls promotions and access to a vast range of perquisites, including housing, the closed system of hospitals, special shops, and so on (for a detailed description see Matthews 1978). The Party uses these resources as prizes to encourage subordinates to compete with each other—possibly "tournament"-style (see Lazear and Rosen 1981) loyally to advance the goals of the Party. When the system functions efficiently, subordinates compete loyally to advance the goals of their superiors by showing initiative, dedication, enterprise, and flexibility (rather than simply obeying orders, as I discuss at much greater length in Wintrobe 1988). In return, the Party ensures that the "implicit contract" of reward for loyal performance is kept, that is, that superiors within the government, ministry, or Party hierarchies do not renege on implicit promises to subordinates. In this way, the Communist party takes the place of enforceable property rights to solve the problem of mutual cheating characteristic of exchange when law-based property rights are absent.

One reason that the Party can fulfill this role is that it is a long-lived institution. Thus, exchanges over time (e.g., across generations) are possible. This explains one commonly noted feature of totalitarian regimes, their orientation toward economic growth. Otherwise, it is not obvious why such policies would be in the interest of the regime—the leaders could simply exercise their monopoly power by lavish living. Why should they sacrifice present consumption for a future in which they will no longer be in office and their sons and daughters are unlikely to succeed them? If membership at the top, for example, in the politburo, were transferable through sale, the leaders of the politburo could collect its capitalized value and would have an incentive to preserve it.[7] It is obvious why this route has not been taken. An alternative, however, is provided if the Communist party facilitates exchanges between the young and the old. In that case differences in time preference will be eliminated through trade—the old (the leadership) pursuing growth-orientated policies and the young offering loyal performance in exchange. This explains the peculiar "futuristic" look of all totalitarian parties—the Thousand-Year Reich, the early associations of fascism with the artistic movement known as futurism, and the enormous program of forced industrialization carried out by Stalin.

In addition to enforcing exchange, the

totalitarian Party possesses a number of features that facilitate investments in loyalty to the Party. *First,* unlike a democracy, the Soviet Union has no "free rider" problem with political participation: political participation (one way of investing in loyalty) is demanded, monitored, and rewarded. (For details on these and other practices, see Hough and Fainsod 1979—or Schapiro 1971 for a historical treatment.) *Second,* membership in the Party is an exclusive privilege. Exclusive membership makes expulsion possible, and the threat of expulsion for disloyalty (whether individually or en masse—the Party purge) is obviously a powerful incentive device. *Third,* the party is centrally organized. All "factions" and groups within the Party are officially prohibited in the Party's statutes; communication and decision making are organized along strictly hierarchical lines (the principle of "democratic centralism"). To the extent that these three incentives are effective, loyalty is diverted toward the Party and its hierarchy rather than to other, proscribed political groupings within it or dissident groups outside of it. (From the Party's point of view, dissidence threatens exchanges in the same way that crime threatens property rights and efficiency in a capitalist economy.)

Fourth, the Party encourages and directs loyalty by maintaining and propagandizing an exclusive ideology that promotes the Party's goals and helps establish and codify its reputation. The ideology of Marxism-Leninism is mainly a critique of capitalist societies and does not fundamentally guide (or hinder) the actual functioning of the Soviet system. Its main function is to promote loyalty to the Party. The ideology identifies heroes and villains, glorifies the successes, and minimizes the failures, of the Party. It may succeed in this respect irrespective of whether it is believed or not if ideological propaganda builds the Party's reputation in the same way that a firm's advertising promotes its reputation (and brand loyalty) in Klein and Leffler's (1981) analysis. In their model it is not the content of advertising but its volume (the accumulated stock) that provides information. However, that repetitive quality that Klein and Leffler (following Nelson 1970) saw as the key to understanding the function of advertising in the West is surely even more characteristic of totalitarian ideology and propaganda. That is, it is not the content of an ideology but its repetition (the magnitude of the Party's sunk investments) that contributes to its reputation and promotes loyalty. For example, why else would *Pravda* devote three-quarters of its space for nine months to the publication of greetings to Stalin on the occasion of his seventieth birthday (Bialer 1980, 66)? Presumably, it is not the content ("Happy Birthday, Stalin!") but the repetition of the message that is the message. *Fifth,* the Party controls the distribution of rents. One simple interpretation of the legendary "shortages" characteristic of Soviet-type systems—otherwise rather difficult to explain (the difficulties have been discussed extensively in Kornai 1980)—is simply that the shortages create rents, which are controlled by the Party. Consequently, they are an excellent device for building Party loyalty. In this interpretation the shortages are a permanent feature of Soviet-type systems, which no economic reform can be expected to remove.

This brief description of the workings of the system (I give more detail and explain why loyalty within the system tends to corrode over time in Wintrobe 1988) suggests that in Soviet-type societies (and, I suspect, in other totalitarian societies) the "organizational technology" for operating an economic system on what might be called loyalty-based property rights is far more advanced than it is in other political systems.

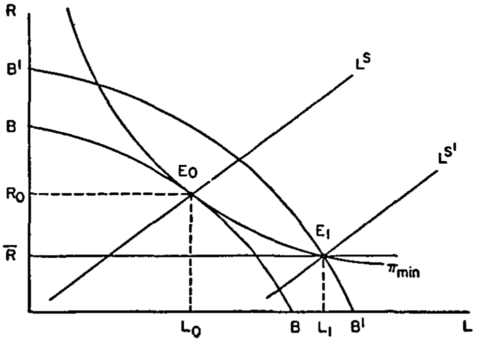
Policy Implications

Perhaps surprisingly (in view of its simplicity) the model developed here does yield rather strong implications with respect to the policies that should be followed by democratic countries vis-à-vis both tin-pot and totalitarian dictatorships. I assume throughout that the only aim of Western governments is to promote freedom; that is, that the countries of the West have no strategic or economic interests in these countries other than the promotion of freedom for their people. This is obviously an idealistic point of view, but it does represent the oft-stated aims of much of Western policy.

Policy toward Tin-pots

The major policy issue with respect to tin-pot dictatorships is whether to extend economic aid to them or not and, if aid is given, whether it should be tied to improvements in the human rights record of such regimes. Figure 5 displays the policy dilemma with respect to tin-pot regimes. The initial equilibrium is at E_0. Consider first the policy of giving aid with no strings attached. Aid in this form does not change the relative costs of repression and loyalty to the dictator. It does imply that more resources (*B'B'* rather than *BB* in Figure 5) could be spent on repression and loyalty while maintaining the dictator's personal consumption at its initial level. However, if, as depicted, the original budget constraint provides enough resources for the dictator to remain in office (it is at least tangent to $\pi_{\min}$), the dictator will simply spend the aid on personal consumption. If the tin-pot could not remain in office without foreign assistance (i.e., if *BB* were everywhere below $\pi_{\min}$), the aid would have the effect of maintaining the dictator in office as long as it continued. The equilibrium levels of R and L at R_0 and L_0, respectively, would, however, be unchanged.

Figure 5. Optimum Western Policy Toward a Tin-pot Regime



An alternative policy often advocated is to "tie" the aid to a constraint on violations of human rights by the tin-pot. Assuming that this constraint can be made effective, that is, that violations can be properly monitored and that the constraint is binding, this implies that the tin-pot will be constrained to impose a level of repression lower than R_0 (e.g., one no greater than $\bar{R}$ in Figure 5). As Figure 5 clearly shows, the effect of the binding constraint is to destabilize the regime. In the short run, loyalty is fixed at L_0; and with $\bar{R}$ of repression and L_0 of loyalty, the dictator has insufficient power to stay in office no matter how high the level of external assistance.

Neither of these two policies, then, is effective in reducing repression. The first policy (aid with no strings attached) is ineffective because although it may maintain the tin-pot in office, it creates no incentives for the tin-pot to substitute loyalty for repression as a means of staying in office. The second policy (aid with a binding human rights constraint) is ineffective because it gives the tin-pot no chance to do so but rather puts the tin-pot in immediate danger of being deposed. (If the aim of the policy is to cause the tin-pot's downfall, why extend aid at all?) Moreover, both policies may have other, counterproductive effects. For example, if the aid is continued for a long time, the tin-pot is freed from the dependence on the loyalty of supporters and may substitute for this a dependence on foreign aid, especially if it is easier to get the aid than to maintain loyalty. Alternatively, if the tin-pot is deposed, the new regime may be even less palatable (Kirkpatrick's point).

There is, however, a third policy that has some chance of success. This is a policy of aid combined with a long-term human rights constraint, that is, one that becomes successively more stringent over time. The aim is to force the tin-pot to a new long-run equilibrium at E_1. The long-run constraint ensures that the tin-pot will in the future only be able to stay in office at a level of repression no greater than $\bar{R}$. The aid provides the tin-pot with sufficient resources to "purchase" the extra required loyalty at no sacrifice to personal consumption, and the long-term nature of the constraint implies that sufficient time is allowed for the dictator's investments to yield the required support. Consequently, this policy alone among those considered provides the correct incentives to the tin-pot to reduce repression. Of course, a number of other policies could be considered. These include the use of economic sanctions, giving aid to international agencies rather than to the tin-pot, "earmarking" the aid to specific investment projects, and so on. Their effects can easily be analyzed within the present framework.

Policy toward Totalitarians

The two central issues for the democracies in dealing with totalitarian governments have been, (1) Should democracies engage in economic relations (trade and investment) with them in the knowledge that these relationships are likely to improve their economic performance, or should they pursue policies such as an arms race or economic sanctions, which have, among other possible effects, that of worsening the totalitarian regimes' economic performance? and (2) Should the democracies push for human rights? Again, the point of view in addressing these questions taken here is a scientific one: given the assumptions of our model, which policies will reduce the optimal level of repression chosen by the totalitarian leader?

Figure 4 displays the policy dilemma of the democracies in dealing with totalitarian governments. E_0 is the initial equilibrium. If, as a result of trade or other favorable economic relationships with the democracies, the economic performance of the regime improves and if the regime can take at least part of the credit for this improvement, the supply of loyalty to the regime L^S shifts to the right to $L^{S'}$. The most plausible result (see "Totalitarian Regimes" section above) is a new equilibrium like E_1 in Figure 4, with the dictator obtaining more power and more loyalty and being more repressive. In this case the direct result of a trade or arms agreement is to worsen the political rights of the people (even if it raises their standard of living); so that these policies, by themselves, tend to be ineffective in increasing freedom. On the other hand, a human rights constraint (e.g., one binding at $\bar{R}$) if imposed alone would simply reduce the power of the regime and therefore be unacceptable to the leader. This can be seen in Figure 4. The new optimum for the dictator with a long-term binding human rights constraint of $\bar{R}$ is at E_2, a lower level of power than E_0. Again, however, although neither policy is effective used alone, they can be effective in combination. The principle is simple: a long-term binding human rights constraint can be suggested to the regime, provided it is accompanied by the offer of a trade agreement or arms agreement or other agreements to improve performance sufficiently to allow the dictator to increase loyalty to L_3. At these levels of repression and loyalty (E_3 in Figure 4) the dictator has as much power as before. Repression, however, is successfully reduced from R_0 to $\bar{R}$). Consequently, a slightly better trade or arms agreement will *increase* the dictator's power and therefore be personally acceptable, while still reducing the level of repression under the regime.

A more acute dilemma confronts the democracies in dealing with totalitarian dictatorships that are in decline and seeking to "reform." The analysis in Figure 4 suggests, as indicated earlier, that the proper response to economic decline for a totalitarian government is some relaxation of repression (e.g., from R_1 to R_0 in Figure 4). The next step in the sequence, however, is economic reform, not full political liberalization. Substantial liberalization (any $R < R_0$) will simply reduce loyalty (and power) further. Moreover, if the leader expects economic reforms initially to cause widespread hardship (e.g., via unemployment or price increases) and hence potential for political unrest, the leader's problems would only be exacerbated by political liberalization (which allows the opposition to organize and bandwagon effects to develop). Consequently, the optimal sequence of reforms for the leader of a totalitarian regime is some relaxation of repression followed by economic reforms to increase the supply of loyalty *before* major political reforms are attempted. Given this important result, the dilemma for the democracies is that if the economic reforms are successful and the leader can claim the credit for this so that loyalty increases in the long run, any power-maximizing leader will then be tempted to renege on promises and respond by *increasing*, rather than decreasing, political repression (since the new, power-maximizing equilibrium at higher L—L_1 compared to L_0—implies higher R). Again, therefore, from the point of view of the democracies that are considering whether to expand economic relations with such a regime, there is no substitute for the principle of insistence on a long-term, binding human rights constraint if their objective is to minimize the repressiveness of the dictator's regime.

Conclusions

I have attempted to use basic tools of economic theory to construct a simple model of the behavior of dictatorship. Two extreme cases were considered: a tin-pot dictatorship (whose dictator wishes only to minimize the costs of remaining in power so as to continue to collect the fruits thereof) and a totalitarian dictatorship (whose leader maximizes power over the population). Some novel predictions were derived. For example, an improvement in economic or other type of performance implies that a tin-pot dictator will reduce the extent of repression of the population; a totalitarian, on the other hand, tends to respond by increasing it. (I present some tentative evidence supporting this prediction for the totalitarian.) The model also explains why military dictatorships (a subspecies of tin-pots) tend to be short-lived and often voluntarily hand over power to a civilian regime. Finally, the model also explains a number of features of totalitarian regimes (e.g., the persistence of shortages in Soviet-type regimes, the preferences of their leadership for economic growth, their fair success in achieving that goal, and the structure of totalitarian party organization).

Perhaps surprisingly, the simple model presented here does yield rather strong policy implications with respect to how democratic regimes should deal with dictatorships, assuming that the democracies wish to maximize freedom in the world. In particular, an emphasis on the observance of human rights under either type of regime (provided it is imposed in the right way and accompanied by sufficient aid, trade, or investment) is shown to be not only a sensible but a necessary condition if economic relations with either type of regime are to increase, not decrease, human freedom.

Notes

Previous versions of this paper were given at the 1988 meetings of the Public Choice Society, at the Institute des Sciences Politiques (Paris), the Universities of Toronto and Western Ontario, and Yale University. I am grateful to participants in those seminars, and especially to David Andolfatto, Peter Howitt, John McDougall, Raj Kumar Sah, Pierre Salmon, and Bob Young for helpful comments and suggestions.

1. Similar incentives to invest in loyalty exist in other situations where there are gains from exchange but no enforceable property rights (e.g., within bureaucracies). Breton and Wintrobe 1982, chap. 4 contains a detailed analysis of the accumulation of loyalty or trust within bureaucracies. (See also Wintrobe and Breton 1986.)
2. See, esp., Arendt 1951, 323-26 and the discussion in Burrowes 1968.
3. See the literature on the political business cycle, which relates political popularity to various macroeconomic indexes, such as the growth in per capita real disposable income. Hibbs 1987 is a good survey of this literature. Important recent contributions by Alesina and others are described in Alesina 1988b.
4. See, e.g., Burrowes 1968 and references therein.
5. Where budgetary considerations do pose a binding constraint, the reader can easily verify that a totalitarian regime that maximizes power subject to a budget constraint will respond in exactly the same way to exogenous changes in economic performance as a regime subject to a loyalty constraint.
6. The main problem is that the émigrés are often atypical. For example, 85% of them are Jewish, and 30% read *samizdat* (banned underground literature).
7. This point is also made in Schap 1988.

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COALITIONS AND CABINET GOVERNMENT

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The formal study of coalitions is active in Europe, whereas the formal study of political institutions preoccupies American scholars. We seek to integrate aspects of these two bodies of research. For nearly thirty years models of coalition government have focused more on coalition than on government. Thus, these theories are essentially extensions of the theory of voting in legislatures. Unlike passing a bill or "dividing a dollar," however, forming a government is not the end of politics but the beginning. During the formation process, rational actors must entertain expectations of subsequent government behavior. We provide a model of rational expectations with an emphasis on the credibility of the policy promises of prospective government partners as determined by the allocation of portfolios in the new government. Portfolio allocation becomes the mechanism by which prospective coalitions make credible promises and so inform the expectations of rational agents in the coalition formation process.

Despite the nearly thirty years of work inspired by William Riker's *The Theory of Political Coalitions* (1962), there is still no authoritative theoretical account of coalition government in parliamentary democracies. The reason for this is that government coalitions have been treated as a special type of legislative coalition, and the study of government coalitions has thus been treated as an extension of the theory of voting in legislatures. This has meant that discussions of government coalitions have concentrated almost exclusively on the fact that they are *coalitions* and more or less ignored the fact that they are also *governments.* Little attention has been devoted to what happens *after* a government has been formed or to how rational expectations about what will happen influences the formation process itself. Consequently, theories of coalition formation have operated without a clear conception of what, precisely, is being formed; and theories of payoff distribution have operated without a clear conception of what, precisely, is being distributed. It is our contention that no significant progress can be made without a consideration of rational forecasts of the behavior in office of prospective multiparty governments.

The account we propose is based on the idea that the credibility of proposals for alternatives to the incumbent government is central to coalition bargaining. Our argument is that the notion of credibility depends crucially on the proposed allocation of cabinet portfolios in the new government. This argument forms the basis of our model. We analyze coalition bargaining over credible alternatives, formally treat strategic issues, and apply the main features of our approach to a real

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world example of coalition bargaining. Finally, we review some of the outstanding issues in coalition theory upon which we believe our model throws light.

We assume that the actors are trying to move government policy outputs as close as possible to their own preferred policies. This is by no means the only motivational assumption that we could have made. In the argot of coalition theory, ours is, broadly speaking, a model driven by *policy-based,* rather than by *office-based,* preferences (Laver and Schofield 1990). While we intend to explore the impact of different assumptions in later work, we take this to be a reasonable starting point.[1]

Cabinet Portfolios and Credible Proposals for Government

Division of labor within a cabinet means that most ministers are assigned particular policy jurisdictions, or "portfolios." These give a minister the job of initiating and implementing policy within a particular field. The resources commanded by a minister to facilitate this—typically control over a government department—give considerable de facto power over policy outputs in the minister's jurisdiction. It is very difficult to implement policy in the face of active opposition from the relevant minister or even to develop a detailed policy alternative.

The role of the cabinet in the business of parliamentary government is what makes the formation of government coalitions quite different from the formation of legislative coalitions. This is because the allocation of cabinet portfolios is much more than a "mere" payoff to be determined by coalition bargaining. Rather, the cabinet is part of the essential definition of the government that forms. It is what Bagehot (1936, 68) called "a combining committee—a *hyphen* which joins, a *buckle* which fastens, the legislative part of the State to the executive part of the State." In many respects the group of cabinet ministers, taken as a whole, *is* the government.

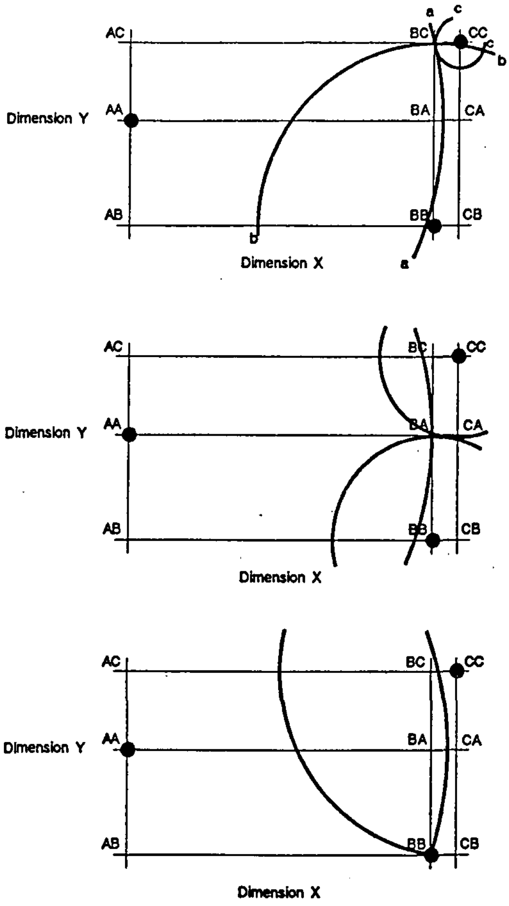
This characterization of the workings of the division-of-labor cabinet system should lead actors to forecast that jurisdiction-specific policy outputs will tend toward those preferred by the party of the relevant minister.[2] This means that a proposal that promises to enact the preferred policy position of the person (party) nominated for each relevant portfolio is *credible* in the sense that it depends only on giving ministers the power to do what they expressly want to do. Any proposal promising that a minister with wide-ranging power over the relevant policy jurisdiction will act against expressed preferences is less credible.

Proposals for government, then, are *policed* by the mechanism of portfolio allocation. Restricting analytical attention to credible proposals has a major impact on the modeling of government formation: there is only a finite number of possible portfolio allocations among parties forming a government. The finite number of credible proposals, furthermore, may be quite small if the number of salient policy jurisdictions is low, the number of parties small, and internal party discipline high.[3]

Given these assumptions, the panels of Figure 1 show, as three black dots, the ideal points of three parties in a system in which two dimensions of policy are salient (ignore the indifference curves for the present). Figure 1 also shows the nine credible policy proposals that can be made in this system.[4] Each is labeled with two letters. The first identifies the party controlling the relevant portfolio on the horizontal dimension (x); the second identifies the party controlling the relevant portfolio on the vertical dimension (y). Thus, point *AB* in the southwest corner of each panel describes a proposal to enact policy *AB*, "policed" by giving party *A* the portfolio controlling the x dimension and party *B* the portfolio controlling the y dimension. Obviously the ideal point of a party, *AA* for example, represents a credible proposal; this gives all relevant portfolios to the party in question, allowing it to implement its ideal policies. Since a proposal is credible only if it is policed by the right portfolio allocation, the point *AB* in Figure 1, for example, represents a credible proposal if *and only if* party *A* gets portfolio x and party *B* gets portfolio y. If party *C* is nominated for one of these portfolios, then the point *AB* is not a credible proposal; its promise of policy output *AB* is cheap talk. More significantly, *AB* is not credible if party *A* gets portfolio y and *B* gets portfolio x. In this very important sense, *AB* and *BA* represent utterly different coalition governments, even though they involve precisely the same coalition partners.

An approach based on credible proposals is, as we shall see, far more tractable than the conventional spatial model, which is based on the assumption that all possible policy proposals are feasible. The latter approach, ignoring the credibility of proposals, allows for a continuum of potential alternatives to the status quo on each policy dimension. Its account of government coalition formation is in the form of a game of weighted voting in a legislature in which the entire policy space constitutes the set of feasible policy outcomes (McKelvey and Schofield 1987; Schofield 1986). On our account, however, in which the only credible proposals are those policed by portfolio allocations, the lattice of credible proposals is a finite set. Furthermore, proposals for alternative governments are associated in an essential manner with the *specific* actors who are needed to police them.

Figure 1. Ideal Points and Win Sets of Selected Credible Proposals



Bargaining over Credible Alternatives to the Status Quo

In this section, we consider what it takes, in strategic terms, to replace an incumbent government in a parliamentary democracy. All strategies must of course operate within a procedural constraint that the executive maintain the confidence of the legislature.[5] While defeated if it loses a legislative confidence vote, a government is not replaced until an alternative wins an (actual or implicit) investiture vote. Thus, any attempt to replace the incumbent government must ultimately pit a proposed alternative against the status quo in the legislature. Only if the alternative wins at this stage will it replace the status quo. Proposals for alternative governments are not, however, disposed of when they are defeated in the legislature. They may be proposed again at any time (and thus may be distinguished from amendments to a legislative bill which, once defeated, are ordinarily laid to rest). In this sense, and in contrast to parliamentary procedure surrounding the act of legislating, the ordering of an agenda of proposals for the formation of a government is not particularly consequential.[6]

We take a *proposal for a government* to consist of a proposed allocation to particular parties of cabinet portfolios having jurisdiction over salient policy dimensions. A proposal may in practice also involve explicit policy promises, but (as suggested above) these will be discounted by rational actors unless they are consistent with expectations produced by portfolio allocations. Thus, the essence of any proposal is the portfolio allocation it involves. For this reason, we say that what is *credible* about a proposal—and what will be implemented—is the ideal policy of the party nominated for each of the relevant portfolios. Thus, in Figure 1 there are nine credible proposals, {*AA*, *AB*, *AC*, *BA*, *BB*, *BC*, *CA*, *CB*, *CC*}.

We say that the parties nominated for portfolio allocations are *participants* in that proposal. Since a party cannot be forced to accept a portfolio, we say that it must give its *assent* to a portfolio assignment—and hence to a proposal in which it is a designated participant—before that proposal can be considered in the legislature. Any one of the participants, by withholding assent to a portfolio assignment, can prevent a proposal from being considered by the full legislature.

There is always an incumbent government. This is the status quo that will prevail if no alternative government forms. It consists of a set of policy positions and portfolio assignments much like those in any proposed alternative. We call the implicit proposal represented by the existing portfolio allocation of the incumbent government x^0. Since x^0 is the "reversion" point when attempts to form an alternative government fail, it is important to be clear about its meaning.

We treat x^0 like any other proposal, credible only if policed by a particular portfolio distribution. Thus, if party *A* holds the portfolio governing the first two policy dimensions, *B* the third, and *C* the fourth and fifth in an incumbent coalition government, x^0 may be written as *AABCC*, where the projection of *A*'s ideal point determines the first two components, *B*'s determines the third component, and *C*'s the fourth and fifth components. In this interpretation, x^0 is composed of the current ideal point components of existing coalition partners according to the jurisdictions of their respective portfolios. As with any proposal, it is assumed, conservatively, that government parties are unable to promise credibly to do anything other than follow their respective policy positions (as announced, say, in the most recent party manifesto) in the ministries under their control. Under this interpretation x^0 must be one of the nine credible proposals in Figure 1.[7]

An alternative can replace the status quo only if it is credible and is preferred by a legislative majority; otherwise, the status quo is retained. For two credible proposals, x and x', representing either the current or some alternative government, we say that x' *wins* against x if and only if x' is preferred to x by a legislative majority. The set of credible proposals that win against x is the *credible win set of* x, or $W(x)$. We hereafter refer to this simply as the win set of x.[8] Thus, $W(x^0)$ is the set of credible proposals that can win against the status quo in a legislative vote.

Figure 1 shows the win set for three of the nine credible proposals, assuming that any two parties comprise a decisive legislative coalition. This set is generated by the intersections of three indifference curves, labeled *aa*, *bb*, and *cc*, respectively. The interior of indifference curve *aa* is the set of all policy points that party *A* prefers to, say, *BC* in the top panel. Curves *bb* and *cc* have parallel interpretations. The credible win set consists of the credible proposals in the union, over all winning coalitions, of intersections of the interiors of winning coalition members' indifference curves. In the examples, then, the credible proposals in the union of the intersections of the interiors of *aa* and *bb*, *aa* and *cc*, and *bb* and *cc* (where the indifference curves are drawn through *z*). In the top panel the credible win set of *BC*, *W*(*BC*), contains one credible proposal, *BA*. In the bottom panel of the figure, *W*(*BB*) contains two credible proposals, *BC* and *BA*. Finally, in the middle panel, *W*(*BA*) contains no credible proposal.

Since every proposal requires the assent of its participants, one proposal cannot defeat another simply by obtaining a majority against it in the legislature. Consider a case where x' can win against x, but at least one of the participants in x' actually prefers x to x'. In this event, some of these participants in x' may not assent to it, thereby preventing it from being considered by the legislature. We say that a proposal x' is considered by its participants to be an *improvement* over x if all of them prefer it to x. Call the set of such Pareto-improving proposals the *improvement set of* x, or $I(x)$. From the bottom panel of Figure 1, we note that proposals *BC* and *BA* are in the credible win set of *BB* but are not in the improvement set of *BB*, since party *B*, a participant in each of the proposals, does not prefer them to *BB*.

For a proposal x, if there are counterproposals that not only win against x in the legislature but also are seen by their participants as improvements over x, we say that they *contend* against x. The set of such proposals is called the *contender set of* x, $C(x) = W(x) \cap I(x)$. In the top panel of Figure 1, *BA* both wins against *BC* and is preferred to *BC* by both its participants. Thus, *BA* is a contender against *BC*.

We should note that not all proposals that contend against x^0 may be serious threats to it. We assume that all actors are endowed with intelligent foresight and thus can anticipate future events. Consequently, actors can anticipate the effects of supporting a proposal that contends against the status quo and is in turn contended against. Imagine, for example, that an alternative x' contends against x^0 but that an alternative x'' contends against x'. The incumbent government may not fall to x' if actors (who otherwise prefer x') fear x' will subsequently fall to an even worse alternative, like x''. Thus, on contemplating the replacement of a government, the actors must anticipate and solve a potentially complex "proposal game."

It is quite possible for three proposals, x, x', and x'' to cycle—with x' contending against x, x'' contending against x', and x contending against x''—in an analogue to voting cycles.[9] Therefore, we adopt a conservative position and regard any status quo that faces contenders as being *vulnerable*. More generally, we say that any proposal x is vulnerable if it would be vulnerable as the incumbent government. However, a status quo that has contenders and thus is vulnerable may nevertheless survive. If, on the other hand, no credible proposal contends against x^0 we say that the status quo is *invulnerable*. More generally, we say that any proposal x is invulnerable if it would be invulnerable as the incumbent government.

The notion of invulnerability provides a link between our approach and the recent model proposed by Austen-Smith and Banks (1990).[10] The "restricted portfolio core" defined by Austen-Smith and

Banks is, in our terms, the set of invulnerable proposals. Put more precisely, the maximal elements of the dominance relation defined by contending are exactly the restricted portfolio core. Austen-Smith and Banks (1990) have shown that a restricted portfolio core always exists with three parties but may not exist with four or more. In our terms, this means that there will always be at least one invulnerable proposal in a three-party system but that there may be no invulnerable proposal in a larger party system.

The vulnerability or invulnerability of proposals for government, including the implicit proposal to retain the current government, is thus the result of the interaction of two processes. The first is the ability of those controlling a legislative majority to replace the status quo. The second is the ability of any participant to withhold assent from any proposal to do this. It is a concern with the interaction of these processes—majoritarian decisiveness and partipant veto power—and with the requirement of credibility that separates our approach (and that of Austen-Smith and Banks) from existing coalition-theoretic accounts of government formation in parliamentary democracies.

It is quite possible for more than one proposal to be invulnerable at the same time. This can readily be seen in Figure 1, where both *BB* and *BA* are invulnerable proposals. The middle panel shows that *BA* is invulnerable, since no credible proposal wins against it in the legislature. The bottom panel shows that *BB* is invulnerable, since the only proposals that win against *BB* do not contend against it (both contain one participant—party *B*—who prefers *BB*). Two proposals x and x' can simultaneously be invulnerable only if x is preferred to x' by a legislative majority but x' is preferred to x by at least one participant in x.[11]

To sum up, the notion of credibility imposes two very significant restrictions on bargaining over the formation of government coalitions. In the first place, only some proposed alternatives to the status quo are credible. In the second place, the credibility of these proposals is not a general property but depends on the participation of certain actors. Thus, only those credible proposals that are preferred to the status quo by their participants are taken seriously in coalition bargaining. The need for credibility thereby empowers veto groups.

Proposal Games

As we have argued, the permanent political question before most parliamentary democracies is whether the incumbent government will continue to govern. In this section we describe, in terms of the circumstances that confront an existing government, the possible strategic interactions produced by this question. In our taxonomy, which we call *proposal games,* there are three circumstances: (1) x^0 is invulnerable; (2) x^0 is vulnerable to invulnerable proposals; and (3) x^0 is vulnerable only to proposals that are themselves vulnerable.

An Invulnerable Status Quo

Game 1. The win set of the status quo contains no credible proposal. Consider again the middle panel of Figure 1. If *BA* is the status quo, no credible proposal can win against it in the legislature. Its credible win set is empty and *BA* cannot be replaced. Put as a proposition, *If no credible proposal wins against the status quo, the incumbent government continues in office.*

This follows directly from the assumption that only credible proposals can be considered as replacements for the status quo and that only proposals that can win against the status quo in the legislature can replace it. This proposition encapsulates the real departure of our account of coalition formation from conventional spatial analyses. While the conventional win set of the status quo in multidimensional policy spaces is generally nonempty, there will often be situations in which this set contains no *credible* proposal, particularly if the incumbent government occupies a central position in the policy space. Indeed, as any reader who plays around with a few examples will quickly see, the conventional win sets of some incumbent governments can be quite "large" and yet contain no *credible* alternative. It should be remarked, moreover, that this proposition does not depend on the number of parties, the dimensionality of the space, or the unidimensionality of portfolio jurisdictions.

Game 2. The status quo is invulnerable; that is, the status quo faces no contender. By definition, the status quo is invulnerable when its contender set, $C(x^0) = W(x^0) \cap I(x^0)$, is empty. Game 1 illustrates one way in which this may happen, that is to say, $W(x^0) = \varnothing$. However, this is not the only way. (Thus game 1 is a special instance of game 2.) Even if the win set of the status quo contains credible proposals, these may not be preferred to the status quo by all of their participants.

An example of a status quo invulnerable in this sense is given in the bottom panel of Figure 1. A status quo of *BB* is not vulnerable to *BA* or *BC* despite the fact that these are credible proposals that win against it in the legislature. This occurs because party *B* can refuse to assent to any proposal in which it participates. Stated as a second proposition, *If the status quo is invulnerble, the incumbent government continues in office even if there are credible proposals that win against it.*

Each of the above games concerns circumstances in which the status quo is invulnerable to proposals and the situation is relatively clear cut. We should stress once more than even these two very simple games imply a lot more stability in political bargaining than is implied by the conventional spatial model. As we shall see in the application to follow, real world governments that would be diagnosed as unstable by conventional spatial analysis are diagnosed as stable under our model, quite simply because nothing contends against them.

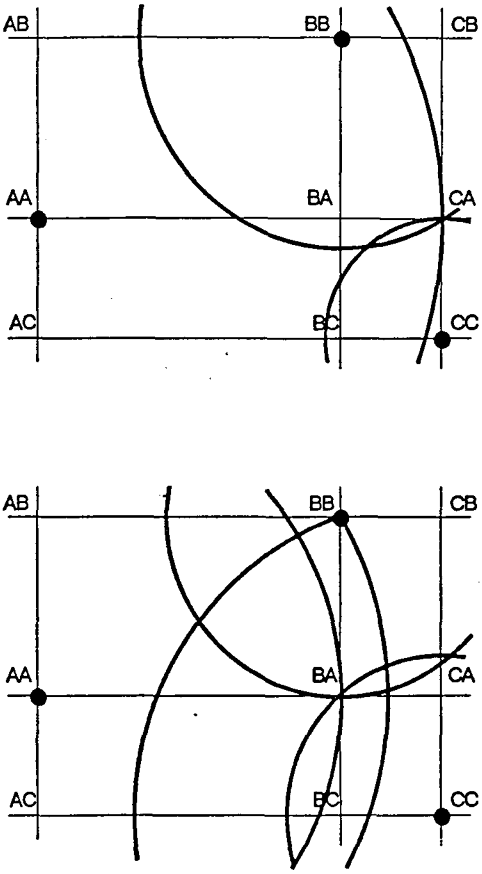
A Status Quo Vulnerable to Invulnerable Contenders

Game 3. The incumbent government faces a single invulnerable contender. This possibility is described in the top panel of Figure 1. If the incumbent government is *BC*, this faces a single contender, *BA*. The middle panel shows that if *BA* becomes the status quo, it is invulnerable. Given this, it is difficult to see how a proposal for *BA* can fail against *BC*. Parties *B* and *A* both prefer *BA* to *BC*, and they control the legislative majority to implement this. No other proposal has this property.

More generally, let the unique invulnerable proposal that contends against the status quo be x^*. This means that x^* wins against the status quo and is preferred to the status quo by its participants (i.e., x^0 is vulnerable to x^*), that no credible x' contends against x^* (i.e., x^* is invulnerable), and that no other proposal shares these properties (i.e., x^* is the unique invulnerable contender against x^0). From the invulnerability of x^* it follows that it will be stable as the new status quo if it replaces x^0 This means that those who prefer x^* to x^0 need not fear that x^* will subsequently be replaced by something that they like less than x^0. They thus have no reason not to vote for x^*. Consequently, *If the incumbent government faces a single invulnerable contender, this contender will replace the status quo.*

Game 4. The incumbent government faces two invulnerable contenders. We

Figure 2. A Mexican Standoff



consider the case in which there are exactly two invulnerable contenders to x^0. The general case is given in the Appendix. Two proposals can be simultaneously invulnerable. If there are two invulnerable proposals, x and x', one (say, x') must be preferred to the other by a legislative majority (we ignore ties throughout). This means that at least one participant in x' must prefer x. (If no participant in x' preferred x and x' is preferred to x by a legislative majority, x' would contend against x and the latter thus would not be invulnerable, contrary to hypothesis.) Whenever some of the participants in x', where x' is one of the contenders against the status quo, prefer another contender to it, we say that x' is *flawed.* Note that one of the two invulnerable proposals is always flawed and that the flawed proposal wins against the unflawed proposal in the legislature.[12]

When a flawed and an unflawed proposal contend against the status quo, two types of conflict of interest are generated. First, there is a direct conflict of interest among the participants in the flawed proposal, of whom some prefer the unflawed proposal to it and some do not. Second, there is a conflict between the participants in the flawed proposal who prefer the unflawed proposal and a legislative majority that prefers the flawed proposal. The latter conflict can be thought of as a Mexican standoff.

An example of a Mexican standoff is illustrated in Figure 2. Two invulnerable proposals, *BA* and *BB*, contend against the status quo, *CA*. Both are preferred by legislative majorities to *CA*, and both are preferred to *CA* by their participants (see upper panel). Proposal *BA* beats *BB* in the legislature but is flawed in the sense that party *B*, one of its participants, prefers *BB* (see lower panel, where $BA \in W(BB)$). While party *B* can announce that it will never assent to *BA*, it cannot credibly commit to this strategy. Thus, *BA* remains an active possibility. Party *A* can announce that it will only vote for *BA*, which it prefers to *BB*, and will always vote against *BB*. Of course, it cannot credibly commit to this strategy either, and *BB* remains an active possibility. If party *A* holds out for *BA*, refusing to vote for *BB*, while party *B* holds out for *BB*, refusing to assent to *BA*, the status quo, *CA*, continues. In this event, both *A* and *B* lose out, since they both prefer either *BB* or *BA* to *CA* but there is a direct conflict of interest between them over which to select. All that we can say, without knowing the specific circumstances of the case, is that either *BB* or *BA* will replace *CA* or neither will and *CA* will remain in force. In short, we have a classic conflict of interest in a bargaining context that, without further explicit modeling, remains indeterminate. This argument, as established in the Appendix, generalizes to any number of contenders.[13]

A Status Quo Vulnerable Only to Vulnerable Contenders

In the first class of proposal games above, the status quo government was vulnerable to *no* proposals. In the second class, it was vulnerable to *invulnerable* proposals. In each case the game "ends" with an invulnerable status quo (sometimes the original one) or with some generalization of a standoff. If the latter, the status quo persists until the deadlock is broken, after which we are again at equilibrium with an invulnerable status quo. In each of these situations, the actors can employ their perfect foresight (looking down the game tree, so to speak) and engage in backward induction. In the last class of proposal games to be considered, the only proposals to contend against the status quo are themselves vulnerable. Looking down the game tree in this instance, the actors see only contenders against the status quo that are themselves vulnerable to counterproposals.

Game 5. The status quo is vulnerable only to vulnerable contenders, and there is no invulnerable proposal. In assuming that there is no invulnerable proposal, we note that it is necessarily the case that there are *contender cycles,* since every proposal loses in the legislature to some alternative that is preferred by its participants. In short, when there is no invulnerable proposal, the dominance relation described by contending has no maximal element. This is precisely the case in which the Austen-Smith and Banks restricted portfolio core is empty. This class of proposal games is also the closest analogue in our taxonomy to the "chaos" found in the conventional majority-rule spatial model, as portrayed in the theorems of McKelvey and Schofield (McKelvey 1976, 1979; McKelvey and Schofield 1987; and Schofield 1983).

Our model of coalition behavior, like these others, has little to say about how the chaos is resolved. In principle, contextual factors—institutional structure and procedure—will provide some of the answer; so, too, will behavioral norms. But no proposed government is stable in the sense that it faces no contenders. With the vulnerability of all potential governments apparent, the life of governments may be short and elections frequent.[14]

Game 6. The status quo is vulnerable only to vulnerable proposals, but an invulnerable proposal exists. This case is to be distinguished from its predecessor, because here there is an invulnerable proposal, x^* (a maximal element of the contending relation, that is, an element of the restricted portfolio core); however, this does *not* contend against x^0, that is, $x^* \notin W(x^0) \cap I(x^0)$. There are two cases to consider, each with a different consequence depending on the manner in which x^* fails to contend against the status quo.[15]

Suppose, first, that the invulnerable proposal beats the status quo, $x^* \in W(x^0)$. Because it does not contend against x^0, it must then be the case that at least one participant in x^* prefers x^0, i.e., $x^* \notin I(x^0)$. The issue is, Can x^* be blocked? We provide one instance in which the answer is *no.* We know that x^0 is vulnerable (say, to x') and that x' is, in turn, vulnerable. To simplify the example, suppose x' is vulnerable to x^*. A majority, by assumption, would like to replace x^0 with x^*. If all the participants in x' are part of this majority, preferring x^* to x^0, and if the participants in x^* prefer it to x', then x' can serve as a threat; that is, at the first stage the majority wanting x^* supports x', to which the participants of x'

assent (since they, too, want x^*). At the second stage this same majority backs x^*, which is again assented to since its participants prefer it to x'. In effect, a majority can credibly threaten to replace x^0 with x', which forces the participants in x^* to put the latter forward. The threat, given perfect foresight, need not even be carried out.

In this special case, in which the status quo is only vulnerable to vulnerable contenders, we see that an invulnerable proposal that does not contend against the status quo may be capable of replacing it. While some participants in x^* prefer x^0, they cannot prevent x^* even if they look down the game tree in a sophisticated manner. This is because they are not participants in the proposal, x', that contends against x^0. Thus, they cannot block x' and, once x' replaces x^0, they will be forced to accept the need to assent to x^*. This is a good example of the strategic complexity in government formation games that is generated by the improvement relation.

Suppose, however (and this is our second case) that the invulnerable proposal x^* does not win against the status quo, that is, $x^* \notin W(x^0)$. In this case a sophisticated majority can anticipate the consequence of voting for proposals on an equilibrium path to x^* and defeat them. This means that x^* is strategically irrelevant and that the logic of game 5 applies.

For game 6, then, we draw the following conclusion: *When there is an invulnerable proposal that does not contend against the status quo, its impact on the course of play is nevertheless predictable. If a majority prefers it to the status quo, there may be paths by which to reach it that cannot be blocked by veto groups. If a majority does not prefer it to the status quo, x^* is strategically irrelevant. In this latter event x^0 may or may not be retained according to the logic of game 5.*

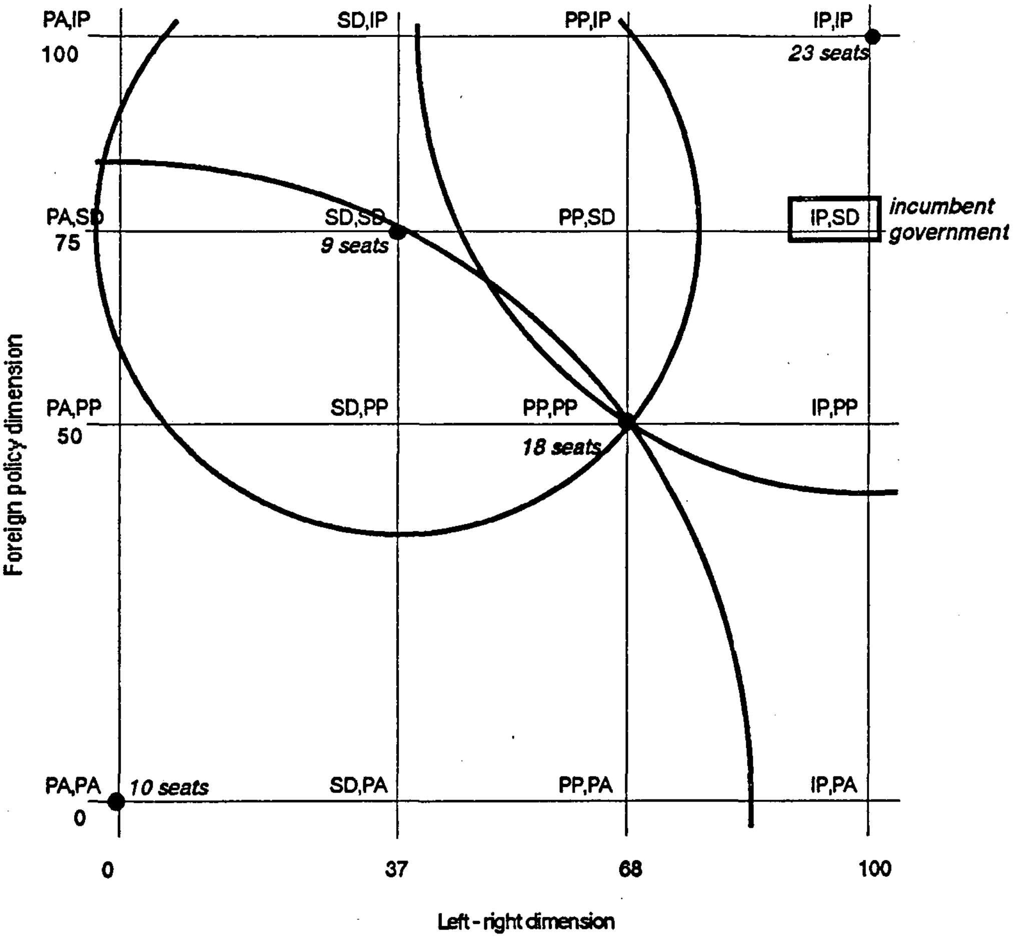
As the analysis of these simple examples has hinted, proposal games can generate complex strategic problems and the number of potential cases and subcases is huge. Lest we lose sight of the forest for the trees, it is worth restating our general line of argument. The reason our approach can do what the conventional spatial model cannot do is that it takes as its fundamental premise that the endpoint of the process is the formation of a government rather than merely a legislative coalition. In this regard we have underscored the vital significance of credibility, participation, and assent to emphasize the various forms of veto power that exist in so-called majority-rule parliaments. The dominance relation generated by the notions of the credible win set and the improvement set can be used to specify proposal games that model the process of government formation and maintenance. Our approach thus predicts equilibrium where the conventional spatial model does not; but it by no means rules out the possibility of instability.

In order to illustrate the difference that all of this makes in the analysis of real world coalition bargaining, we turn now to a particular example. It shows how an analyst who wishes to apply the approach that we suggest can explore real world coalition bargaining in a relatively straightforward manner.

Application: A Mexican Standoff in Iceland

Coalition politics in Iceland in 1967 and 1971 provides good illustration of the processes we describe. Iceland, on most accounts, has two salient dimensions of policy, the conventional left-right dimension on economic matters and a foreign policy dimension related to Icelandic membership in NATO and the U.S. base at Keflavik (Grimsson 1982; Laver and Schofield 1990). The two key dimensions are policed by two key cabinet portfolios, Finance and Foreign Affairs. Four parties won seats in the Althingi (legislature) in

Figure 3. Ideal Points and Credible Policy Proposals in Iceland, 1967

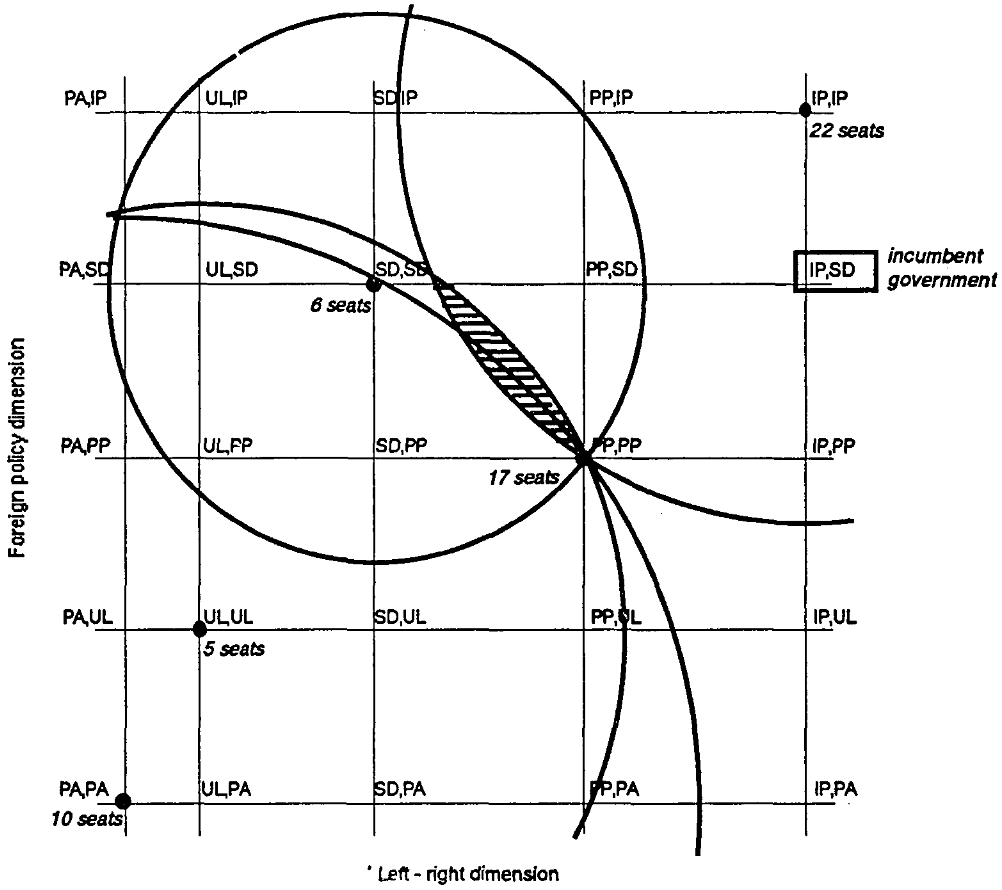


1967, the People's Alliance (*PA*), the Progressive party (*PP*), the Social Democratic party (*SD*), and the Independence party (*IP*). They were joined by a fifth in 1971, the Union of Liberals and Leftists (*UL*). Party policy positions and the number of seats won by the parties in 1967 and 1971 are given in Figures 3 and 4, respectively, which also indicate all credible proposals available.[16]

In 1967 the incumbent government was (*IP, SD*), a coalition between *IP*, controlling Finance, and *SD*, controlling Foreign Affairs. As Figure 3 shows, four credible proposals contended against this: (*PP, SD*), (*SD, PP*), (*SD, SD*), (*PP, PP*).[17] Two of these proposals are invulnerable: (*PP, SD*) and (*PP, PP*).[18] Of the two invulnerable proposals, (*PP, SD*) is flawed since *PP* prefers (*PP, PP*) (obviously); yet (*PP, SD*) is in the win set of (*PP, PP*), being preferred to it by the winning coalition of *SD* and *IP* (this win set is displayed in Figure 3).

There is thus a Mexican standoff proposal game if *PP* holds out for (*PP, PP*)

Figure 4. Ideal Points and Credible Policy Positions in Iceland, 1971


PA,IP
UL,IP
SD,IP
PP,IP
IP,IP
22 seats
PA,SD
UL,SD
SD,SD
6 seats
PP,SD
IP,SD
incumbent government
PA,PP
UL,PP
SD,PP
PP,PP
17 seats
IP,PP
PA,UL
UL,UL
5 seats
SD,UL
PP,UL
IP,UL
PA,PA
10 seats
UL,PA
SD,PA
PP,PA
IP,PA
Foreign policy dimension
Left - right dimension

while *SD* holds out for (*PP, SD*). In this event, *SD* should be supported by *IP*. If *PP* refuses to assent to (*PP, SD*), while *SD* refuses to vote for (*PP, PP*), the status quo, (*IP, SD*), continues. This is in fact what happened. Note that *PP* can do better by assenting to (*PP, SD*) and *SD* can do better by voting for (*PP, PP*), but there is a direct conflict of interest between them over who should give in.

In 1971, as Figure 4 shows, a new party, *UL*, entered the system, changing the set of winning legislative coalitions and increasing the number of credible proposals from 16 to 25. As we have just seen, the status quo was (*IP, SD*).[19] After the 1971 election, six proposals contended against this: (*PP, SD*), (*SD, PP*), (*PP, PP*), (*SD, SD*), (*PP, UL*), (*SD, UL*). We note that the last of these contenders is generated by the "surplus" majority coalition (*PP, IP, SD, UL*); *SD* or *UL* may be surplus to the *legislative* majority of this coalition but are clearly necessary for the credibility of its policy position. There is a single invulnerable proposal, (*PP, PP*). Moreover, (*PP, PP*) contends against all other proposals—it now contends against (*PP, SD*) because of the emergence of the decisive coalition (*PP, UL, PA*). It is very difficult, therefore, to see how a proposal for (*PP, PP*) can be resisted. This is in fact the proposal that prevailed. While the full executive coalition that took office comprised (*PP, UL, PA*), it was the Progressive party that controlled the two key portfolios.

Discussion: Taking Credibility Seriously

Having laid out the broad structure of our approach and provided an example of its application, we now discuss some of the general ways in which it might add to our understanding of the politics of government coalitions. For the most part we confine ourselves to discussing salient substantive consequences of taking credibility seriously. We conclude, however, by pointing to how the broad structure may be developed.

Minority and Surplus Majority Governments

As we hinted above, our approach casts the analysis of minority and surplus majority government in a completely new light. For a proposal to be able to replace the status quo, as we have seen, it must be a credible contender. While a proposal can contend against the status quo only if it is preferred by a legislative majority, this in no sense requires that the participants in the proposal themselves constitute a legislative majority. On this account, we treat minority governments, which we formally define as "governments whose participants do not constitute a legislative majority," in exactly the same way as any other government. In no sense should they be treated as exceptional cases, idiosyncracies, or pathologies. Indeed, our approach can identify circumstances in which minority governments are more likely to form. When a single invulnerable proposal contends against the status quo, the participants may not themselves constitute a legislative majority. Nonetheless, nothing can contend against a minority government comprising only these participants.

Just as our approach can accommodate minority governments, it can also accommodate surplus majority governments. A party may be vital to the *credibility* of a proposal for a government even if it is "surplus" to its legislative majority. A party is redundant only if removing it from a proposal affects neither the proposal's credibility nor its vulnerability. This means that the well-established notion of the "minimal winning" coalition must be reconsidered when applied to *government* coalitions.[20]

Entry and Exit

Another matter illuminated by taking credibility seriously is the birth and death of political parties. The entry of a new party into the system generates many new credible proposals, while the exit of a party removes credible proposals from consideration. Since there are n^m credible proposals in a system of n parties and m portfolios, the number of credible proposals is very sensitive to entry and exit. For a new party to have an impact within our general approach, however, it must be capable of adding credibility to a proposal for a government. Its policy preferences must be known and it must contain politicians of the calibre to be cabinet ministers. Neither condition may be fulfilled by a new, small party immediately on formation. A significant split within an established party, however, may instantly transform coalitional politics if it takes place on the basis of a well-documented policy dispute involving politicians with clear cabinet potential.

Cabinet Reshuffles

A third important consequence of taking credibility seriously is the fact that credible proposals are not anonymous. Depending as it does upon specific portfolio allocations, the fundamental basis of a government is altered by reallocations of portfolios, or "cabinet reshuffles" (as they are usually known). Returning to Figure 1, we see that *AB* and *BA* represent *quite* different credible proposals, being almost as far apart as any two proposals in the space even though they involve the same two participants. If a government *BA* forms, reversing the allocation of portfolios is, in our terms, tantamount to changing the government, as the new government holds out the promise of quite different policy outputs. Furthermore, there is no reason to believe that a reshuffled government will be stable just because its predecessor was. In our example, *BA* is an invulnerable status quo; but *AB* faces a wide range of credible contenders.

Extensions

Our approach to the analysis of coalition government here is based on a series of simplifying assumptions. These simplifications raise many interesting issues, which we can do no more than allude to now and take up in later work. They can be grouped into several broad categories.

First, there are the motivational assumptions of the model. The approach here is based on an assumption of policy-seeking behavior by politicians. Our approach could, however, be extended to incorporate office-seeking motivations, which may well serve to explain why parties seek portfolios other than those that control key policy dimensions. If parties value both office and policy payoffs, those losing out in the policy-setting game may be paid off with "minor" portfolios. Once we incorporate office-seeking motivations and broaden our horizon to look at the interaction of electoral competition and coalition bargaining (as in Austen-Smith and Banks 1988 and Laver 1989), this approach offers potentially rich possibilities for analyzing the behavior of parties that modify policy positions for strategic reasons so as to put credible contenders in the win set of a hitherto invulnerable government. We could thereby develop a theory of coalition government that provided an account of strategic behavior by opposition parties.[21]

Perhaps the weakest theoretical feature of our model is the presumed inability of parties in government to "cooperate." A second set of potential extensions of our approach has to do with stipulating alternative sources of credibility. We have thus far conceived of credibility as attaching to proposals for government in a fixed, exogenous fashion. The possibility of some endogenous development of credibility constitutes an important next step in the extension of our approach. We might, for example, consider the development of trust between government parties as they attempt to move away from the credible (and coalitionally inefficient) proposal that forms the original basis of their coalition toward a (more efficient) policy position closer to their contract curve. And if this is possible, we must allow for its anticipation in government formation negotiations.

Third, note that we have talked of parties as if they were unitary actors. This approach, however, can be modified to take account of the effects of intraparty politics (Laver and Shepsle 1990). For example, there may be several credible policy positions that could be generated by a given party, each policed by a prominent party politician. We could then explore the potential impact of intraparty politics on coalition bargaining and, indeed, of coalition bargaining on intraparty politics.

Finally, there is a set of potential extensions to this approach that entail additional institutional assumptions. These might, for example, take account of the special agenda-setting role of the prime minister; the constraints imposed by the Finance Ministry, cabinet committees, and doctrine of cabinet responsibility on the discretion of other ministers; the role of party leaders in determining the allocation of portfolios within parties; the endogeneity of ministries and their jurisdictions; and the impact of constitutional provisions for government formation and dissolution on reversion points in coalition bargaining. Most importantly, perhaps, a proper modeling of institutional features will cause us to relax the strong assumption of policy dictatorship by ministers in the direction of some weaker form of agenda power. This, in turn, will require incorporating a temporal dimension, and hence subsequent elections, repeat play, and reputations.

This list could be extended almost indefinitely. The central point, however, is that dealing with governments rather than merely with legislative coalitions gives us the freedom to consider a range of issues that are central to the operation of coalition government. Existing approaches to the analysis of coalition government cannot do this since they do not take seriously the fact that once a government coalition assumes office, it becomes a government with the power to do more or less what it wants on a surprisingly large range of issues.

Appendix

In this appendix we establish the general case in which an incumbent government faces k invulnerable contenders. It is established for $k = 3$, but the argument holds for any k.

Game 4′. The incumbent government faces k invulnerable contenders. For $k = 3$ suppose $x, y, z \in C(x^0)$. By the assumption of invulnerability we have three conditions: (1) x invulnerable $\rightarrow y \notin W(x) \cap I(x)$ and $z \notin W(x) \cap I(x)$, (2) y invulnerable $\rightarrow x \notin W(y) \cap I(y)$ and $z \notin W(y) \cap I(y)$, and (3) z invulnerable $\rightarrow x \notin W(z) \cap I(z)$ and $y \notin W(z) \cap I(z)$. With conditions 1–3 we consider two cases, depending upon whether or not x, y, and z are in a win set cycle.

Case 1. Suppose there is no win set cycle involving x, y, and z; then $x \in W(y)$, $y \in W(z)$, and $x \in W(z)$. Combining this with our assumption of invulnerability implies that $x \notin I(y)$ (from condition 2), $x \notin I(z)$ (condition 3), and $y \notin I(z)$ (condition 3); that is, whenever one of these alternatives beats another, it cannot also be an improvement over the other for all its participants. What we have is a generalization of the Mexican standoff in which various actors face off against one another. For any pair of these proposals, on the one side stands a majority favoring one over the other, while on the other side stands at least one participant in the winning proposal who prefers the other to it. In this otherwise unstructured bargaining setting, almost anything can happen. Should assent for any of the contenders be forthcoming for some reason or another, the status quo is replaced and the game ends with this new alternative as the (invulnerable) status quo government. If, on the other hand, consent is not granted, the status quo may remain in place.

Case 2. Suppose there is a win set cycle among x, y, and z; then $x \in W(y)$, $y \in W(z)$, and $z \in W(x)$. Sparing the details, we simply state the consequences of coupling this profile with the invulnerability conditions above. If x, y, and z are invulnerable, it must be the case that the relation generated by the improvement sets cycles in the opposite direction to the relation generated by the win sets, that is to say, $x \notin I(y)$, $y \notin I(z)$, and $z \notin I(x)$; and once again, as a slightly different kind of generalization of the Mexican standoff (one that could not happen with fewer than three invulnerable contenders), the final result is indeterminate.

Nevertheless, both of these cases permit us to make a nonobvious assertion: with two or more invulnerable contenders to the incumbent government, it is entirely possible for the incumbent government to survive. This outcome is not necessary, but it is certainly possible. To settle the indeterminacy of these proposal games, an explicit bargaining theory must be employed. That the status quo *might* survive, however, is yet another indication of the sort of stability that comes from taking credibility seriously.

Notes

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1. There is a variety of rationales for policy-based preferences. Party politicians may prefer particular policies intrinsically or instrumentally and, if the latter, for current or future electoral reasons.

2. In effect, we are assuming that cabinet ministers are policy dictators in their respective jurisdictions. This assumption is actually defensible in some polities, whereas in others a weaker form of agenda control is the more appropriate assumption. In making the dictatorship assumption, ours is a highly stylized model, surely not the last word. The thrust of many of our conclusions will require modification under weaker assumptions about ministerial power, a point we discuss in the concluding section. In passing, we note the familial resemblance of our approach to models of legislatures in the "new institutionalism" tradition that place a premium on structural features as explanatory elements. We treat cabinet portfolios much like committees in the U.S. legislative context. See Shepsle 1979 and Shepsle and Weingast 1981, 1984.

3. In order to make the presentation of our approach more straightforward, we do indeed begin with these assumptions. For the same reason, we employ examples and illustrations that assume that each jurisdiction is one-dimensional; no novel complications develop if we relax this assumption.

4. We assume Euclidean preferences throughout, so that preferences are measured by the Euclidean distance from an actor's ideal point. This is a crucial assumption, departures from which are discussed in Austen-Smith and Banks 1990.

5. Whether or not this is demonstrated during a formal investiture process, it is always demonstrated by a legislative vote of confidence (or no confidence) in the government. A government is liable to face such a vote any time the legislature is in session. Thus, there is always at least an implicit investiture vote whenever the government is exposed to the possibility of a confidence motion (Laver and Schofield 1990, chap. 4).

6. In their very insightful model of government formation, Austen-Smith and Banks (1988) assume that after an election the head of state selects the largest party as *formateur,* allowing it to try to form a government. If it fails, the second largest party becomes the *formateur.* If all parties (in order of legislative weight) have failed at forming a government, a "caretaker" is appointed until new elections can be scheduled. The modeling advantage of assuming this exogenously defined sequence of proposals is that agents may condition their actions on well-grounded expectations about future developments; consequently, the dynamic programming algorithm of backward induction may be employed. The disadvantage of such an assumption is that it is arbitrary and unrealistic. It is often not the case that a potential government can be dismissed simply because, at some point, it does not command a legislative majority. In Israel in December 1988, for example, the participants repeatedly and categorically "ruled out" a coalition between Likud and Labor. Nevertheless, it continually reemerged in negotiations and was, in the end, the government that actually formed.

7. Alternative interpretations of x^0 are plausible. We mention them in passing but do not employ them further in this study. In general, there are policies that all of the coalition partners of a government would prefer to x^0 (as defined in the text). Party *A*, for instance, might be able to make concessions to *B* on dimensions 1 and 2, *B* might concede to *C* on dimension 3, and *C* might concede to *A* on dimensions 4 and 5 in a manner preferred by all of the parties to x^0. While *promises* to make such concessions are merely cheap talk, members of a long-lived incumbent government may have evolved various credible means of cooperation. Hence, the policy position of the existing government may be taken as credible, whatever it is, simply because it exists. On this latter interpretation x^0 may, in principle, be any point in the Pareto set of the incumbent coalition. This interpretation of the status quo relies on an unmodeled "evolution" of cooperation; the alternative in the text, on the other hand, is at the other extreme, assuming *no* subsequent cooperation. The possibility of cooperation evolving between government parties strikes us as an attractive interpretation and is an obvious place for more work. However, if cooperation among government partners may occur, it is necessary to incorporate rational expectations of

this prospect into the bargaining in the initial government formation process. It is also necessary to model clearly the self-enforcement properties of any "evolved" cooperation, that is, the punishment strategies that maintain it. These are standard requirements in modern noncooperative game theory.

Another possible interpretation of x^0 focuses on what happens if the incumbent government were to be brought down but then could not be replaced. In most parliamentary democracies the resignation of the *prime minister* does not take effect until a new government is installed, though all other ministers are free to resign their portfolios, in effect "turning in their badges." If the government falls and all other parties pull out, the incumbent prime minister's party, holding all the "badges," continues in office. On this third interpretation the reversion point, x^0, is the ideal point of the prime minister's party. In Figure 1, x^0 must be one of the three party ideal points. As in the other interpretation in this note, the game theoretics must be carefully modeled. For example, in "giving up a badge," a government partner must correctly and rationally anticipate the possibility of the prime minister's ideal prevailing in the absence of a new government yet nevertheless see it in its interest to do so.

8. The win set of x, as conventionally used in spatial models, contains *any* point that commands a legislative majority against x. In contrast, the credible win set contains only *credible* points that win against x.

9. Examples are available from the authors.

10. The argument by Austen-Smith and Banks (1990), based on theoretical assumptions nearly identical to our own, was developed simultaneously and independently. As they note, our analysis focuses on contending as a dominance relation, while theirs aims to characterize equilibrium formally. They should be seen as complementary undertakings.

11. Notwithstanding the fact that more than one policy proposal can be invulnerable, no more than one *party ideal point* can be invulnerable. Consider two ideal points, x and x'. Ignoring ties, one ideal point (say, x) must beat the other in the legislature. Each ideal point is, obviously, considered by its (sole) participant to be an improvement over anything else. Thus x beats x' in the legislature and is seen as an improvement over x' by its participant. In other words, x contends against x', and x' therefore cannot be invulnerable. Note that this argument does not imply that some party ideal point is always invulnerable but rather that two or more party ideal points cannot simultaneously be invulnerable.

12. To repeat, if the unflawed proposal were to win against the flawed proposal, it contends against the flawed proposal, and the latter cannot, therefore, be invulnerable.

13. As a final observation, we note that Austen-Smith and Banks (1990) would identify all the invulnerable contenders as elements in the restricted portfolio core. They would identify all such points as equilibria of this particular game. Our analysis differs on this point inasmuch as we suggest that it is also possible for the status quo to survive in this instance. Indeed, our analysis reminds us that the core is "retentive" but need not be "attractive." Hence, even though a restricted portfolio core exists in the case of multiple invulnerable contenders, it need not correctly identify the equilibrium of the government formation process without an additional bargaining theory because of Mexican standoff–like situations. In sum, a possible "falling out among thieves" (the participants in the various invulnerable contenders) may give an otherwise vulnerable status quo government renewed, if tenuous, life.

14. Frequency of elections in this instance is not strictly implied. Indeed, it is institutionally contingent inasmuch as regimes that depart from the Westminster model may find themselves with an "unstable" government forced to serve out its term critically wounded or to reorganize constantly, without the opportunity to call for new elections.

15. Note that contending is an asymmetric relation: $x \in C(x') \rightarrow x' \notin C(x)$. But it is not complete, so that it is entirely possible to have both $x \notin C(x')$ and $x' \notin C(x)$. In the case at hand x^* is invulnerable—that is, $C(x^*) = \varnothing$—but $x^* \notin C(x^0)$.

16. In Iceland the upper chamber contains 20 members, with 11 constituting a majority, whereas the lower chamber contains 40 members, with 21 a majority. For finance bills and parliamentary motions (including confidence votes), the chambers are pooled and a joint division is taken. For this reason Grimsson (1982, 156) argues that 32 seats are needed for a working majority in the pooled chamber. This is the figure that we use, though our results are unchanged if the number is reduced to 31. It is conceivable (though we have not examined the prospect in detail) that some of the winning coalitions we identify would not be winning if a potential need for separate working majorities in both chambers were taken into account. In the figures, party foreign policy positions are taken directly from estimates by Grimsson (1982, 149); we are aware of no other published estimates of these. Left-right economic positions are the means of three sets of estimates, each rescaled to range from 0 to 100 (Dodd 1976; Grimsson 1982; Hardarson and Kristensen 1987).

17. The winning legislative coalition (*PP, SD, PA*) prefers each of these to (*IP, SD*), and each is also an improvement for its participants.

18. (*PP, PP*) contends against (*SD, SD*), while (*PP, PP*) and (*SD, SD*) contend against (*SD, PP*). To see the former, note that $(PP, PP) \in I(SD, SD)$ (since party *PP* prefers it) and $(PP, PP) \in W(SD, SD)$, preferred by the forty-one-seat winning coalition of *PP* and *IP*. The other claims follow by similar arguments.

19. It is worth pointing out again that the fact that (*IP, SD*) survived in 1967 despite facing invulnerable contenders distinguishes our approach from Austen-Smith and Banks 1990. (*IP, SD*) is *not* a maximal element of the contending relation but survived because of the Mexican standoff. Even more incredible, it survived for four years.

20. This should be taken as elaborating rather than contradicting Riker's (1962) seminal insights. For Riker, members of a winning coalition must be necessary and sufficient to its winning. In a pure majority-rule legislature (that is, no agenda control by cabinets, committees, etc.), his size principle describes these necessary and sufficient members. In an institutionally more elaborate legislature, however, a *government* may be smaller than the legislative majority necessary to sustain it (as we have argued). At other times its supporting legislative coalition may contain parties unnecessary to its *legislative majority* but absolutely essential to its credibility.

21. We should emphasize that even in a model with office-seeking motivations, it is important not to *attenuate* the connection between officeholding and policy making. In Austen-Smith and Banks 1988, for example, parties have both policy preferences and office preferences, but they combine additively with the former unaffected by the latter. Portfolios, in their model, are a kind of currency in which parties may be compensated for disappointments in policy and have nothing to do with policy per se. In our view (and in Austen-Smith and Banks 1990), in contrast, portfolio assignments are intimately connected to policy and cannot be regarded merely as sources of compensation.

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STABLE GOVERNMENTS AND THE ALLOCATION OF POLICY PORTFOLIOS

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Different members of coalition governments typically have responsibility for different aspects, or dimensions, of policy. Such responsibilities are allocated as portfolios to government members. Given a distribution of such portfolios, final government policy is derived as the accumulation of individual members' decisions in regard to their respective responsibilities. We develop a portfolio allocation model of government formation and policy decision in multiparty legislatures. In particular, we focus on stable portfolio allocations, where a stable allocation is one that yields a policy that no legislative coalition is willing or able to overturn. Several notions of stability are considered and related to the usual concept of the core. Among the results are that although stable allocations are not guaranteed, such allocations can exist with minority governments; and that final policy outcomes associated with stable governments need not be "centrist."

Despite three decades or so of formal theorizing, our understanding of coalition formation and policy choice in multiparty legislatures remains fairly limited. The set of models proposed so far can be divided roughly into two groups, depending on the set of outcomes and the characteristics of the parties' preferences in the underlying game. In one group are models that analyze the choice of a governmental policy from some well-defined space. In such models the assumption of nontransferable utility is warranted: only one policy position can ultimately be implemented (see Axelrod 1970; Dodd 1976; McKelvey, Ordeshook, and Winer 1978; Schofield 1986; Swaan 1973). The issue then concerns the existence of stable governmental policies—*stable* in the sense of the willingness and ability of competing coalitions to overturn a proposed policy outcome. On the opposite extreme are models that attempt to predict the membership of a government with respect to the allocation of portfolios (e.g., cabinet positions, ministries, etc.) among the represented parties. In these models portfolios are typically characterized as a set of transferable benefits; the issue then concerns the stability of any allocation to alternative allocations proposed by competing coalitions (see Riker 1962; Schofield 1982; Schofield and Laver 1985).

Although all of these authors recognize that parties' preferences depend on both policy and nonpolicy (i.e., portfolio) aspects of government, the implicit assumption is that such issues are orthogonal and hence can be analyzed independently of one another. We would argue, however, that the principal role of portfolios in a parliamentary system is to provide structure to the policy-making pro-

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cess of an existing government in much the same way that committees structure the decision making in the U.S. Congress. Hence, the preferences parties exhibit over different allocations of portfolios will largely be derived preferences, that is, induced from more fundamental policy payoff schedules. The importance of portfolio structure stems from the notion of the portfolio itself, wherein the holder is endowed with certain rights and responsibilities pertaining to a set of policy alternatives or dimensions, rights that can be abrogated only by the explicit removal of the existing government. In particular, "the control of a given set of ministries confers upon a party a particular set of institutional resources which it can manipulate in an effort to advance its policy goals" (Browne and Franklin 1973, 454). Therefore, governmental policy, which we can think of as simply the accumulation of the decisions on all of the portfolio dimensions, will be directly related to the underlying allocation of portfolios, since in general the member parties' preferences over policy, and hence their decision calculus over any portfolio dimension, will differ.

One consequence of portfolios, then, is to inhibit the ability of a coalition, acting as a government, to select from the set of all possible policy outcomes, since the only implementable outcomes for the coalition are those generated by some allocation of portfolios among its members. This is in contrast to the policy choice models, where for any coalition the entire policy space itself constitutes the set of implementable outcomes. On the other hand, the existence of portfolios also limits the ability of a coalition to overturn an existing or proposed government, since the relevant set of alternative outcomes is itself constrained by the requirement that such alternatives be generated by some portfolio allocation. Our goal here is to analyze how such constraints on coalitional behavior affect the theoretical predictions concerning the stability and qualitative features of parliamentary governments.

The definition of portfolios used here is related to the notion of committee systems studied in U.S. legislative politics, where a committee is associated with a particular jurisdiction in the policy space and exhibits certain influence over the outcomes in this jurisdiction. The analogy to a portfolio allocation is then the committee assignment; and, as Shepsle (1979) has shown, given a distribution of committee assignments and a delegation of authority to the committees, there exists "stability," in that a policy outcome emerges as equilibrium behavior among the participants. Here, however, the goal is not simply to characterize the policy outcomes *given an allocation of portfolios* but rather to examine whether there exists stability in the *selection of the allocations.* However, the linkage between these two issues is clear, since the stability of a particular portfolio allocation will be a function of the policy outcome associated with it as well as all competing allocations. We examine the relationship of the current model with the literature on structure-induced equilibrium in greater detail in the conclusion.

Independently of our work, Laver and Shepsle (1990) also study the role of portfolios in the determination of government outcomes in a multiparty setting. Unlike us, however, they do not study stable allocations but rather focus on the dynamics of government formation and dissolution. Their study and this one are thus complementary in their approaches: while Laver and Shepsle consider a particular process, we examine terminal stages of a variety of such processes (although these processes are more or less implicit here).

The Model

Consider a legislature comprising a set of parties, $K = \{1, \ldots, |K|\}$. Each party

j has a legislative weight $1 \geq w_j \geq 0$, where we can think of these weights as being the proportions of legislative seats won in the previous election. A coalition of parties $C \subseteq K$ is decisive in the legislature if and only if $\Sigma_C w_j > 1/2$; let K^d denote the set of decisive coalitions. Each party $j \in K$ is presumed to have strictly quasi-concave preferences $u_j(\bullet)$ over the issue space X, where $X \subset \mathrm{R}^m$ is compact and convex, and $\infty > m \geq 2$. Let $x^j = \operatorname{argmax}_X u_j(\bullet)$, and assume that x^j is interior to X for all j. Thus, a legislature can be described as a vector $\Omega = (K, X, \{w_j\}, \{u_j\})$.

Given a legislature Ω we assume there are two principal stages to the process of selecting an outcome from X. In the first stage a government forms and its members are allocated portfolios, where a portfolio is simply one of the issue dimensions defining the policy space X. Let $J = \{e_1, \ldots, e_m\}$ denote the usual basis for R^m and let 2^J be the power set of J. A *portfolio allocation* is a map α: $K \rightarrow 2^j$, where $\alpha(j)$ consists of the policy dimensions or portfolios associated with party j. We assume that each policy dimension is associated with a single party, so that

$$\bigcup_K \alpha(j) = J$$

and $\alpha(k) \cap \alpha(j) = \phi$, all j, k. Let A denote the set of allocations; since m and K are finite, A is finite as well. For each portfolio allocation α, define the set $K(\alpha) = \{j \in K \mid \alpha(j) \neq \phi\}$ as the government generated by the portfolio allocation α. Since for all α the government $K(\alpha)$ is uniquely defined, we can without loss of generality characterize the output from the first stage as the selection of a portfolio allocation $\alpha \in A$.

In the second stage of the process the parties comprising the government $K(\alpha)$ interact to determine an outcome $x \in X$, where in general such interaction will be a function of the portfolio allocation beyond simply the determination of $K(\alpha)$. In particular, we view party j as possessing a degree of autonomy over the dimensions of the policy outcome x implied by the portfolios $\alpha(j)$; similarly, j is constrained in its ability to influence x in those dimensions not in $\alpha(j)$. To incorporate these behavioral constraints we model x as being the outcome of a noncooperative game played between members of $K(\alpha)$. Although there are various game forms applicable in such an environment, currently we adopt a simple game structure in which the member parties have complete control over the elements of x in their respective jurisdictions. Formally, the game in normal form is $\Gamma(\alpha) = (<S_j>_{K(\alpha)}, <u_j(\bullet)>_{K(\alpha)})$, where S_j is the intersection of the vector subspace spanned by $\alpha(j)$ and X. For any $z \in X$ and $j \in K(\alpha)$, let z_{-j} denote the components of z outside party j's portfolio assignment. Then given any z_{-j}, each party $j \in K(\alpha)$ seeks to

$$\max_{z_j \in S_j} u_j(z_j, z_{-j}).$$

DEFINITION 1. *A policy equilibrium relative to* α *is a vector* $(z_j^*)_{j \in K(\alpha)}$ *such that for all* $j \in K(\alpha)$, $u_j(z_j^*, z_{-j}^*) \geq u_j(y, z_{-j}^*)$ *for all* $y \in S_j$.

By assumption, X is compact and convex; hence, S_j is compact and convex for all j. So by quasi concavity of $u_j(\bullet)$, all J, for all $\alpha \in A$ there exists a policy equilibrium relative to α (Debreu 1952).

Let $Q(\alpha)$ denote the set of (pure strategy) equilibria for any $\alpha \in A$. In general, $Q(\alpha)$ may not be singleton; however, if all parties' preferences over X are separable in all issues, then for every $j \in K(\alpha)$ it is a dominant strategy in $\Gamma(\alpha)$ to select $z_j^*(\alpha) = (<x_r^j>_{r \in \alpha(j)})$, so that $Q(\alpha) = \{z^*(\alpha)\}$ for all $\alpha \in A$. *Assume hereafter that* $|Q(\alpha) = 1$, *all* $\alpha \in A$. Then the payoffs to all parties in the legislature are uniquely described for all feasible allocations $\alpha \in A$. Thus, when we consider the first-stage decision of a portfolio allocation $\alpha \in A$, the parties in the legislature have well-defined preferences over the set A. Equivalently, we can consider the first stage of the process as selecting an outcome from

$$Q = \bigcup_{A} Q(\alpha),$$

where Q constitutes the "implementable" set of outcomes. Note that

$$\bigcup_{K} \{x^j\} \cup Q,$$

so that the ideal points of the represented parties are always implementable. Further, if preferences are separable, $Q = \{x \in X \mid \forall\, r = 1, \ldots, m$ there exists $j \in K$ such that $x_r = x^j_r\}$, so that the set of implementable outcomes Q corresponds to the lattice of points created by the various admixtures of the parties' ideal points.

Finally, we describe the selection in the first stage of a portfolio allocation $\alpha \in A$ or, equivalently, an implementable outcome $x \in Q$. Since the parties' preferences are well defined over the set A, one option is to mimic the behavioral model in the second stage and posit a noncooperative game in which a portfolio allocation is chosen (recent examples of noncooperative models of government formation include Austen-Smith and Banks 1988 and Baron 1989). However, our motivation here is to understand the implications of portfolio assignments in models of coalition formation and government stability. Hence, with regard to behavior in the first stage of the process, we stay with the canonic approach to modeling government selection, namely, the core—the implied emphasis being on the willingness and ability of decisive coalitions to overturn prospective policy outcomes. However, we insist throughout that final payoffs to parties be those induced from the noncooperative policy choice process of the second stage. So while it is assumed that coalitions can freely coordinate behavior with respect to the allocation of portfolios, once an allocation is determined, we presume there is no mechanism available to the coalition to enforce a particular policy decision by any portfolio holder. (If there were such a mechanism, there is little reason to consider portfolio allocations per se at all.)

For any coalition C and any pair of alternatives $x, y \in X$, we say that C prefers x to y (written $x >_c y$) if and only if $u_j(x) > u_j(y)$ for all $j \in C$; and for each $C \subseteq K$, $Z \subseteq X$, let $P_c(Z) = \{x \in Z \mid y >_c x$ for no $y \in Z\}$ denote the set of Pareto optimal points in Z for the coalition C. Recall the usual notion of the core for a legislature Ω: $\mathrm{C}_X(\Omega) = \{x \in X \mid y >_c x$ for no $y \in X$, $C \in K^d\}$. Thus, an outcome x is in the core if there does not exist a decisive coalition that prefers some other outcome to x. In the current model, however, the only points of relevance are those implementable through some allocation of portfolios, that is, Q. Thus, a natural extension of the definition of the core for our purposes would be the following.

DEFINITION 2. $\mathrm{C}_Q(\Omega) = \{x \in Q \mid y >_c x$ *for no* $y \in Q$, $C \in K^d\}$.

(Where Ω is understood, we simply label it C_Q.) Call the set C_Q the *portfolio core.* Thus, if $x \in \mathrm{C}_Q$, x is implementable and there does not exist a decisive coalition that prefers some other implementable outcome to x. Alternatively, if $x \notin \mathrm{C}_Q$, there exists a decisive coalition $C \in K^d$ and an implementable outcome $y \in Q$ such that (C, y) can overturn the outcome x.

Although natural, this definition is not obviously consistent with the presence of portfolios. In particular, an outcome $x \in Q$ may be overturned by the pair (C, y), $C \in K^d$, $y \in Q$, where the coalition C cannot by itself implement the outcome y. Consider the three-party legislature in

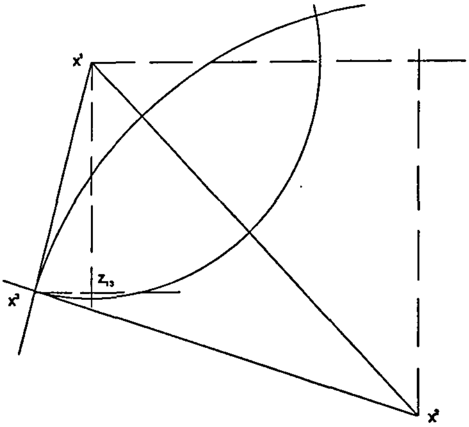
Figure 1, where preferences are Euclidean and $w_j = 1/3$, all $j \in K$. (In all two-dimensional examples the point z_{ij} is the outcome associated with the portfolio that assigns issue 1 to party i and issue 2 to party j.) The implementable outcome x^3 is overturned by $(\{1, 2\}, z_{13})$, since parties 1 and 2 both prefer z_{13} to x^3, and z_{13} is implementable. On the other hand, to implement z_{13} requires $\alpha(1) = 1$ and $\alpha(3) = 2$, so that in essence party 3 would be a member of the government chosen to overturn 3's ideal policy.

An alternative definition for the core in the current model is based on assuming that a decisive coalition can only overturn a prospective outcome with an alternative that is implementable by the coalition itself. For any coalition $C \subseteq K$, define $A_c = \{\alpha \in A \mid K(\alpha) \subseteq C\}$ and $Q_c = \{x \in Q \mid x = z^*(\alpha)$ for some $\alpha \in A_c\}$. Q_c is the set of policies that C can implement by itself. Define the *restricted portfolio core* as follows.

DEFINITION 3. $\mathrm{C}_R(\Omega) = \{x \in Q \mid y >_c x$ *for no* $y \in Q_c$, $C \in K^d\}$.

Thus, C_R is the set of portfolio allocations that cannot be overturned by any decisive coalition acting on its own. Clearly, $\mathrm{C}_Q(\Omega) \subseteq \mathrm{C}_R(\Omega)$, since for any decisive coalition the set of available alternatives with which to overturn an outcome is Q under the former, Q_c under the latter, and $Q_c \subseteq Q$. In addition, there exist legislatures where this inclusion is strict; that is, there can exist an alternative $x \in \mathrm{C}_R$ and a decisive coalition C such that $y >_c x$, $y \in Q$, but $K[z^{*-1}(y)]$ is not a subset of C. For example, in Figure 1 $\mathrm{C}_Q(\Omega) = \{z_{13}\}$ while $\mathrm{C}_R(\Omega) = \{x^3, z_{13}\}$.

Figure 1. C_Q May Not Be Reasonable



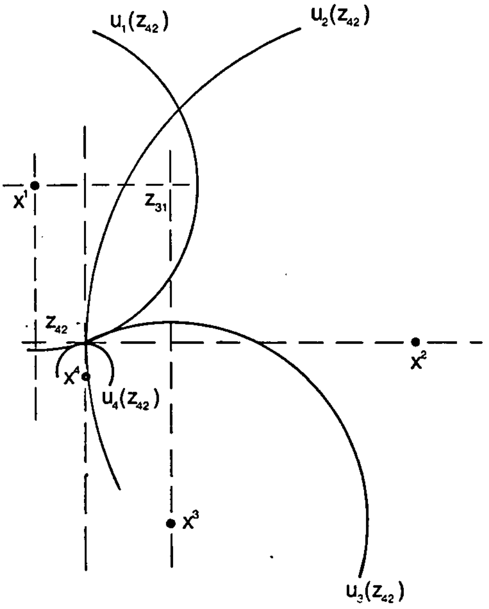
Note: $w_j = 1/3$ for all j; $\mathrm{C}_Q = \{z_{13}\} \subset \mathrm{C}_R = \{x^3, z_{13}\}$.

It is worth noting here that the analysis in Laver and Shepsle (1989) is explicitly based on the dominance relation defining the restricted portfolio core. In their terminology, points in C_R correspond to "invulnerable" status quo policies. Alternatively, we can think of C_R as identifying steady states of a process characterized by the underlying dominance relation.

The stable sets C_Q and C_R represent extreme cases with regard to a decisive coalition's influence: under C_Q a decisive coalition can implement any outcome in Q, while under C_R a decisive coalition can implement only those outcomes in Q that are supported by governments wholly within the coalition. In both cases the ability of a decisive coalition to overturn any given proposal in Q or (equivalently) the set of alternatives to a given proposal that a coalition can implement is independent of that proposal. Such independence may not be reasonable. Instead, implementable alternatives to a proposal by any coalition may be a function of the proposal itself.

Consider the following situation. Suppose there is some $y \in Q$ and some $x \in Q \setminus Q_c$ such that $x >_c y$ for $C \in K^d$. Suppose further that the government associated with x, $K[z^{*-1}(x)]$, differs from that associated with y, $K[z^{*-1}(y)]$, only in that members in $K[z^{*-1}(x)]$ not in $K[z^{*-1}(y)]$ are members of C. Then,

Figure 2. C_Q, C_I, and C_R Are Nested



Note: $w_1 = .49$; $w_j = .17$ for all $j \neq 1$; $C_Q = \phi \subset C_I = \{z_{42}\} \subset C_R = \{z_{42}, x^4\}$.

because C is decisive, it seems reasonable to assume that C can take away portfolios from the relevant members of $K[z^{*-1}(y)]$ and allocate them to members of C to implement x. In other words, C cannot allocate new portfolios to noncoalition members (in contrast to the relation underlying C_Q) but can insist that they surrender or continue to hold portfolios they might currently control. Thus, for the legislature in Figure 1, given the prospective outcome $x = x^3$ the coalition {1, 2} takes the portfolio on the first dimension away from party 3 and allocates it to party 1, thereby generating z_{13} as the outcome.

To make these observations precise, for any $x \in Q$ let $z^{*-1}(x) = \alpha_x$, and, for all $x \in Q$, $C \subseteq K$, define $Q_c(x) = \{y \in Q \mid j \notin C \rightarrow \alpha_y(j) \subseteq \alpha_x(j)\}$. Thus, $Q_c(x)$ consists of the implementable outcomes that the coalition C can generate by taking away portfolios from parties that are in $K(\alpha_x)$ but not in C and allocating them among the members of C. Note that $x \in Q_c(x)$, for all $x \in Q$, $C \subseteq K$.

DEFINITION 4. $C_I(\Omega) = \{x \in Q \mid y >_c x$ *for no* $y \in Q_c(x)$, $C \in K^d\}$.

Clearly,

$$Q_c \subseteq \bigcup_{Q} Q_c(x) \subseteq Q;$$

therefore, for all legislatures, $C_Q(\Omega) \subseteq C_I(\Omega) \subseteq C_R(\Omega)$. Given this set inclusion, call C_I the *intermediate portfolio core.* To see that there exist legislatures such that $C_Q(\Omega) \subset C_I(\Omega) \subset C_R(\Omega)$, consider the legislature described in Figure 2, where $w_1 = .49$ and $w_j = .17$ for all $j \neq 1$ and u_j is Euclidean for all j. Then the decisive coalitions are $K^d = \{\{1, 2\}, \{1, 3\}, \{1, 4\}, \{2, 3, 4\}\}$, and the claim is that $C_Q = \phi$, $C_I = \{z_{42}\}$, and $C_R = \{z_{42}, x^4\}$. To see $x^4 \in C_R$, note that the coalitions {1, 2} and {1, 3} cannot by themselves improve on x^4; and clearly party 4 cannot be made better off. In addition, $z_{42} \in C_R$, since {1, 2}, {1, 3}, and {1, 4} cannot by themselves improve on z_{42}, nor can the coalition {2, 3, 4}. On the other hand, $x^4 \notin C_I$, since now the coalition {1, 2} is better off by taking away the second portfolio from party 4 and giving it to party 2, thus generating z_{42}. Similarly, $z_{42} \notin C_Q$, since the coalition {1, 2} prefers the outcome z_{31} to z_{42}.

The traditional focus on elements of the core underscores the interest in the stability of coalition governments, where the relevant question is, Given the current or "status quo" government policy, does there exist a consensus for change, that is, does there exist a decisive coalition that prefers some other (implementable) policy? The issue in the current model concerning which of the defined core concepts is appropriate clearly depends on this notion of consensus, in particular the amount of tacit agreement required from the members of the proposed government to a change in the status quo. If agreement is required of all such members, C_R is appropriate, since in essence these parties possess a veto power with regard to changes in the existing government. On the other hand, if rejecting membership in the proposed government implies that the relevant portfolios are simply assigned to other members of the government, each party has an incentive to accept any such agreements; the alternative is for some other party to control certain portfolios. This scenario then justifies C_Q, since all parties will tacitly agree to accept any portfolios assigned to them.

Our view is that each of these core concepts can be justified according to different models of how the actual coalitional bargaining takes place, with particular emphasis on the opportunity cost to a party of rejecting a proposed change in the status quo. So one response to the indeterminacy over which core concept is the more appropriate is to attempt a description of the actual process by which coalitions reach agreements. Given that the defined core concepts are nested, however, we can understand general properties of stable portfolio allocations and stable governments by examining the characteristics of all three concepts. In addition, this will shed some light on how different models of the coalitional bargaining process affect the nature and existence of such allocations.

We shall be deriving results on the existence and qualitative properties of the various core concepts. Before proceeding, however, two features of the model just described are worthy of additional attention. The first concerns the second-stage game played by the parties in government, while the second concerns associating dimensions in the policy space with the set of portfolios. Consider these in turn.

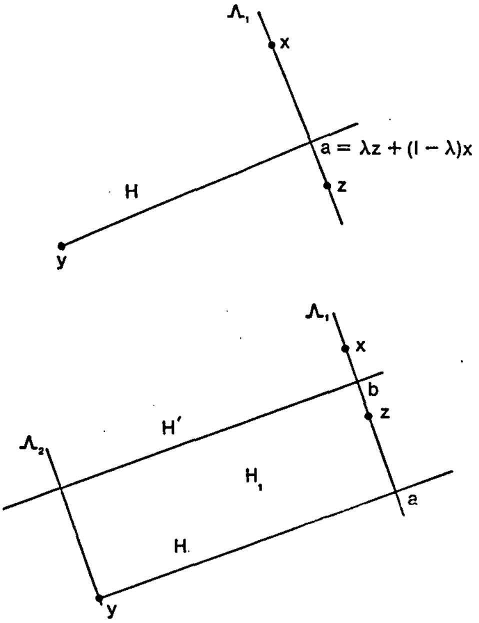
Note in Figure 1 that although z_{13} is implemented by the coalition {1, 3}, z_{13} is not an efficient outcome relative to {1, 3}, that is, z_{13} does not lie on the contract curve between x^1 and x^3. Thus, such circumstances provide collective incentives for the members of the government to act cooperatively to realize these gains. However, at the individual level the incentive will always be for parties to renege on any such agreement, given each party's relative autonomy in the policy dimensions under its control. For example, suppose in Figure 1 that {1, 3} "agreed" ex ante to form a government with the portfolio distribution $\{\alpha(1) = 1, \alpha(3) = 2\}$ and implement an outcome $y = (y_1, y_2)$ on the contact curve $[x^1, x^3]$ that both parties strictly prefer to z_{13}. Such an agreement must entail party 1's choosing $y_1 < x_1^1$ and party 3's choosing $y_2 > x_2^3$. But then given, say, that party 1 selects y_1 as agreed, party 3 can improve its payoff by reneging and selecting $x_2^3 \neq y_2$. And by definition of holding a portfolio, party 3 is able to impose x_2^3. Hence, unless there exists a mechanism whereby parties can credibly commit to behavior that is not individually optimal, outcomes generated by portfolio allocations can and will be collectively suboptimal.

With regard to the second feature, associating portfolios with subsets of issue dimensions is a natural starting point for the study of governmental policy making through portfolio allocations in the context of parliamentary democracies. A substantive implication of doing this is that the orthogonality of issue dimensions imposes orthogonality on the set of portfolios. However, we do not assume that parties are constrained to hold at most one portfolio; and having several portfolios under some party's control is, in many respects, observationally equivalent to that party's holding one or more nonorthogonal portfolios.

Results

Our first series of results concerns existence properties of the portfolio core, in

Figure 3. Proof of Theorem 1 in R_2



that each provides a sufficient condition for C_Q to be nonempty; by the ordering of noted core sets, these imply the nonemptiness of C_I and C_R as well. Clearly, if the legislature Ω is such that there exists a party $j \in K$ with weight $w_j > 1/2$, all of the core concepts will coincide and be equal to $\{x^j\}$; hence, existence is not an issue. Therefore, in the remainder of this section we assume that $w_j < 1/2$ for all $j \in K$.

Theorems 1 and 2 use characteristics of the existing notions of the core to generate sufficient conditions for C_Q to be nonempty.

THEOREM 1. *If Ω is such that u_j is Euclidean for all $j \in K$, $C_X(\Omega) \neq \phi$ implies $C_Q(\Omega) \neq \phi$; however the converse is false.*

Proof: Let $y \in C_X$. There are two cases.

Case 1. There exists $j \in K$ such that $x^j = y$. Consider $\alpha \in A$ such that $K(\alpha) = \{j\}$. Then $z^*(\alpha) = x^j = y \in C_X$, implying $y' >_C z^*(\alpha)$ for no $y' \neq z^*(\alpha)$, $C \in K^d$. A fortiori, $y' >_C z^*(\alpha)$ for no $y' \in Q$, $C \in K^d$. Hence, $y \in C_Q$.

Case 2. $y \neq x^j$ for any $j \in K$. If $y = z^*(\alpha)$ for some $\alpha \in A$, we are done by a similar argument as for Case 1. So suppose $y \neq z^*(\alpha)$ for any α. (Figure 3 illustrates the argument to follow for $m = 2$.) Define the set of issue-by-issue medians:

$$M = \{\mu \in X \mid \forall r = 1, \ldots, m, \sum_{L_1(r)} w_j \leq 1/2 \text{ and } \sum_{L_2(r)} w_j \leq 1/2\},$$

where $L_1(r) = \{j \in K \mid x^j_r < \mu_r\}$ and $L_2(r) = \{j \in K \mid x^j_r > \mu_r\}$. By separable preferences, a trivial generalization of Cox (1987, theorem 1) implies that $[C_X \neq \phi$ and $C_X \cap Q = \phi] \rightarrow y \in M \setminus [Q \cap M]$. In particular, $y \in C_X \leftrightarrow \forall$ lines Λ through y,

$$\sum_{L_i(\Lambda)} w_j \leq 1/2, \; i = 1, 2;$$

where $L_1(\Lambda) = \{j \in K \mid \text{argmax}_\Lambda\, u_j(\bullet) < y\}$, $L_2(\Lambda) = \{j \in K \mid \text{argmax}_\Lambda\, u_j(\bullet) > y\}$. By separable preferences, $Q \cap M \neq \phi$. Consider any $z \in Q \cap M$ such that $\|z - y\| \leq \|z' - y\|$, $\forall z' \in Q \cap M$. We claim $z \in C_Q$; suppose not. Then there exists $x \in Q$ such that $x >_C z$ for some $C \in K^d$. Let Λ_1 be the line through x and z, and let H denote the hyperplane through y orthogonal to Λ_1. Let $\Lambda_1 \cap H = a$; either $a = \lambda z + (1 - \lambda)x$ for some $\lambda \in [0, 1]$ or not. In the first case, construction of z implies $\lambda \geq 1/2$. Therefore, by Euclidean preferences and Cox's theorem, $x >_C z \rightarrow C \notin K^d$. So $\Lambda_1 \cap H \neq \lambda z + (1 - \lambda)x$ for any $\lambda \in [0, 1]$. Therefore, x and z lie in the same open halfspace defined by H: $x, z \in H_1$. Let $b = (1/2)(x + z)$ and let H' be the hyperplane orthogonal to Λ_1 such that $H' \cap \Lambda_1 = b$. Let Λ_2 be the line through y orthogonal to H'. Clearly, $H' \subset H_1$ and Λ_2 parallel to Λ_1. By Euclidean preferences, therefore, $x >_C z$ and $C \in K^d \rightarrow$ there exists $i \in \{1, 2\}$ such that

$$\sum_{L_i(\Lambda_2)} w_j > 1/2.$$

But this contradicts $y \in C_X$, by Cox's theorem.

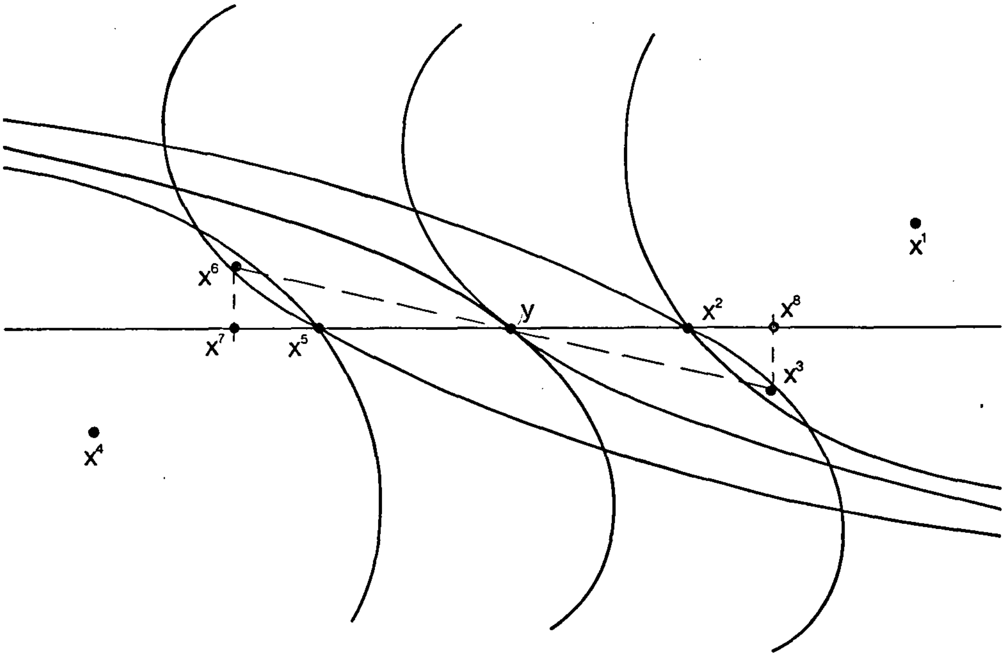
To see that the converse implication is false, it is enough to consider Figure 1 above. QED

Thus, whenever preferences are Euclidean and there exists a core in the usual sense, there exists a portfolio core and hence an intermediate and a restricted portfolio core.

The assumption of Euclidean preferences is essential to Theorem 1, as Figure 4 below demonstrates. In this example $X \subset R^2$; $K = \{1, \ldots, 8\}$; $w_1 = w_4 = .2$; $w_j = .1$ for all $j \neq 1, 4$; parties $j \neq 1, 4$ have Euclidean preferences; and parties 1 and 4 have separable but *not* Euclidean preferences. It can be checked that $y \in C_X$ and $C_Q = C_I = C_R = \phi$.

The key to this example of course is that the core point y is not implementable, since if it were, it would necessarily be in Q and hence in C_Q as well. However, if attention is confined to *structurally stable* cores, $C_X^* \subseteq C_X$ (McKelvey and Schofield 1986; Schofield 1986), a different result holds. Structurally stable cores are core policies that remain core policies under small variations in the distribution of preferences. In particular, such cores do not require the usual Plott-type conditions (1967). The distinction between structurally stable and unstable cores is important because, as Schofield (1986) and McKelvey and Schofield (1986) have shown, in weighted voting games structurally stable cores can exist. Thus, whereas generic existence is still not

Figure 4. The Necessity of Euclidean Preferences for Theorem 1



Note: $w_1 = w_4 = .2$; $w_j = .1$ for all $j \neq 1, 4$; $M = [x^5, x^2]$ and $Q \cap M = \{x^5, x^2\}$. $C_X = \{y\}$, but $C_Q = \phi$ because $x^5 >_{c} x^7 >_{c'} x^6 >_{c''} x^5$ and $x^2 >_{d} x^8 >_{d'} x^3 >_{d''} x^2$ where $c = \{1, 2, 3, 5, 8\}$, $c' = \{2, 3, 4, 5, 7\}$, $c'' = \{1, 4, 6, 7\}$, $d = \{2, 4, 5, 6, 7\}$, $d' = \{1, 2, 5, 6, 8\}$, and $d'' = \{1, 3, 4, 8\}$ and all these coalitions are decisive.

guaranteed, when equilibrium outcomes do exist, they are typically robust, that is, perturbations of the preferences do not destroy existence.

THEOREM 2. *For all legislatures* Ω, $\mathbf{C}_X^*(\Omega) \subseteq C_Q(\Omega)$.

Proof: $y \in \mathbf{C}_X^*$ implies there exists $i \in K$ such that $y = x^i$ (Schofield 1986); but $x^j \in Q \; \forall j \in K$, so that $y \in Q$. QED

Thus all structurally stable core outcomes constitute portfolio core outcomes as well. Theorem 2, along with such examples as Figure 1, imply that the behavioral constraints provided by portfolio assignments tend to generate more structurally stable outcomes. This is consistent with the literature suggesting that institutions are designed for the purpose of creating such stability (see, e.g., Riker 1980).

The next result shows that if there exist only three parties, all of which have Euclidean preferences, the portfolio core is nonempty.

THEOREM 3. *Suppose* Ω *is such that* $|K| = 3$, *and* u_j *is Euclidean* $\forall j \in K$; *then* $\mathbf{C}_Q(\Omega) \neq \phi$.

Proof: If $\mathbf{C}_X \neq \phi$, we are done, by Theorem 1. So assume $\mathbf{C}_X = \phi$ and let $\alpha \in A$ be such that $z^*(\alpha) = \mu \in M$. Since $\mathbf{C}_X = \phi$, $C \subseteq K$ is decisive iff $|C| \geq 2$; and $\alpha(j) \neq J$ for any $j \in K$. We claim $\mu \in \mathbf{C}_Q$; suppose not. Consider $C = \{1,2\}$ and assume there exists $y = z^*(\alpha) \in Q$ such that $y >_c \mu$. Surely, $y \notin M$. Let Λ_j be the line connecting x^j and y, $j = 1,2$ and let H be the contract curve between x^1 and x^2. By Euclidean preferences, $y = (<x_r^1>_{r \in \alpha'(1)}, <x_s^2>_{s \in \alpha'(2)}, <x_t^3>_{t \in \alpha'(3)})$ and $\alpha'(j) \neq J$, for all j. First suppose $\alpha'(j) \neq \phi$, $j = 1, 2$. Then either Λ_1 and Λ_2 are orthogonal or $y \in H$. Since $\mu \in M$ and $y \notin M$, $y \in H \rightarrow u_j(\mu) > u_j(y)$ for some $j = 1, 2$. So Λ_1 and Λ_2 must be orthogonal. Let Λ_y be the line through y orthogonal to H, with $\zeta x^1 + (1 - \zeta)x^2 = \Lambda_y \cap H$. Similarly, let Λ_μ be the line through μ orthogonal to H, with $\xi x^1 + (1 - \xi)x^2 = \Lambda_\mu \cap H$. Because Λ_1 and Λ_2 are orthogonal and μ is an issue-by-issue median, $\zeta, \xi \in (0, 1)$ necessarily. By Euclidean preferences, $u_1(y) > u_1(\mu)$ iff $\zeta > \xi$; and $u_2(y) > u_2(\mu)$ iff $\zeta < \xi$. Hence $y >_c \mu$ is impossible. Now suppose $\alpha'(1) = \phi$. Then Λ_1 and Λ_2 are not necessarily orthogonal, given $y \notin H$. If $\Lambda_1 \bullet \Lambda_2 > 0$, the previous argument applies a fortiori. If $\Lambda_1 \bullet \Lambda_2 < 0$, $u_j(\bullet)$ separable for all j implies there exists $y' \in Q$ such that $y' >_c y$ and $C \subseteq K[z^{*-1}(y')]$. In this case, we can repeat the previous argument, substituting y' for y throughout, to yield the desired contradiction. Therefore, $\mu \in \mathbf{C}_Q$ as claimed. QED

As in Theorem 1 the assumption of Euclidean preferences in Theorem 3 cannot be relaxed to, for example, arbitrary separable or "elliptical" preferences. However, concluding that Euclidean preferences are necessary, in that perturbing away from such preferences would surely eliminate existence, would be incorrect. For example, if preferences are separable, Q is defined by the admixtures of the parties' ideal points regardless of whether preferences are Euclidean. Thus, since Q is a discrete subset of X, sufficiently small perturbations in preferences away from Euclidean that maintain separability will almost never upset a portfolio core. For example, in Figure 1, if preferences are separable, z_{13} is in Q and is a stable outcome so long as preferences are "close to" being Euclidean.

On the other hand, even with Euclidean preferences, Theorem 3 cannot be extended to legislatures with more than three parties. Figure 5 illustrates this assertion for a four-party legislature where $w_4 = .49$ and $w_j = .17$ for $j = 1, 2, 3$. Here $\mathbf{C}_R = \phi$, as is easy to check. Examples for $|K| \geq 5$ and arbitrary weights in which $\mathbf{C}_R = \phi$ are likewise easy to construct.

The next result gives a sufficient condition on the distribution of the parties' ideal points for C_Q to be nonempty.

THEOREM 4. *Let $i\colon K \rightarrow J$ be a function, and $\lambda_j \in \mathbf{R}$. Suppose all parties' preferences are Euclidean. Then $x^j = \lambda_j e_{i(j)}$ for all $j \in K$ implies $\mathrm{C}_Q \neq \phi$.*

Proof: If $\mathrm{C}_X \neq \phi$, the result follows from Theorem 1. Suppose $\mathrm{C}_X = \phi$ and, $\forall\, i = 1, 2, \ldots, m$, define $D_i = \{C \in K^d \mid \forall\, j \in C,\ x^j = \lambda_j e_i \text{ and } \forall\, j, j' \in C,\ \lambda_j\lambda_{j'} > 0\}$. Since $C \cap C' \neq \phi$, $\forall\, C, C' \in K^d$, either $D_i = \phi\ \forall\, i$ or there exists exactly one $i \in J$ for which $D_i \neq \phi$. In the first case, Euclidean preferences and definition of x^j, $j \in K$, together imply $0 \in P_c(Q)\ \forall\, C \in K^d$. The theorem now follows from Theorem 5.1 below. Without loss of generality, suppose in the second case that $D_1 \neq \phi$. Let $C_1 = \{j \in C \mid C \in D_1\}$ and let t be an order statistic on C_1 such that $\|x^{j(t)}\| \leq \|x^{j(t+1)}\|$, $\forall\, t = 1, 2, \ldots, |C_1|-1$. Let $C_{-1} = K \setminus C_1$. Since $C_1 \in K^d$, $C_{-1} \notin K^d$. Define $D(t) = \{C \in K^d \mid C \subseteq (C_{-1} \cup \{j(1), \ldots, j(t)\}) \text{ and } C \cap C_{-1} \neq \phi\}$.

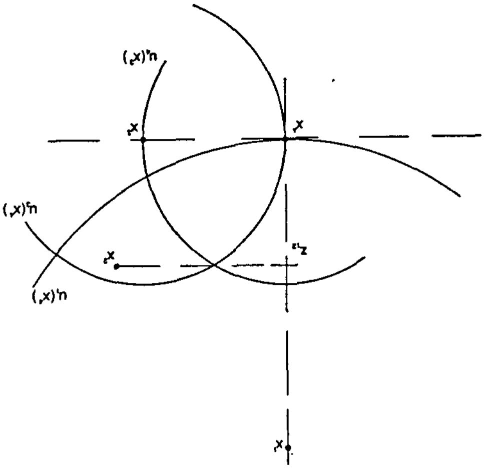
$$\text{Let } D(t^*) \equiv \min_t \{D(t) \mid D(t) \neq \phi\}$$

and choose any $D^* \in D(t^*)$; such a decisive coalition surely exists. Clearly, $x^{j(t^*)} \in P_{D^*}(Q)$. Indeed, by construction of D^*, Euclidean preferences and definition of x^j, $j \in K$, $\{x^{j(1)}, \ldots, x^{j(t^*)}\} \subset P_c(Q)$, $\forall\, C \in K^d$ such that $C \cap C_{-1} \neq \phi$. Therefore, $x^{j(t^*)} \notin P_D(Q)$ for some $D \in K^d$ implies $D \subseteq C_1 \setminus \{j(1), \ldots, j(t^*)\}$. But then $D^* \cap D = \phi$, which contradicts $D \in K^d$. Hence, $x^{j(t^*)} \in P_c(Q)$ for all $C \in K^d$, and the desired result follows from theorem 5.1.

QED

In words, Theorem 4 says that if, in the legislature, each party's ideal point lies on some basis vector in J, the portfolio core is necessarily not empty. It follows immediately from this result that if, for all j, $k \in K$, $x^j \cdot x^k = 0$ and preferences are Euclidean, we can choose an orthonormal basis J' for $\mathbf{R}^m$ other than J, such that C_Q defined with respect to J' is nonempty even though C_Q defined with respect to J *is* empty; that is, theorem 4 suggests that portfolios may not, in contrast to the specification here, necessarily be associated with the usual basis for $\mathbf{R}^m$.

Figure 5. Theorem 3 Cannot Be Extended to More Than Three Parties



Note: $w_4 = .49$; $w_j = .17$ for all $j \neq 4$; $M = \{x^4\}$, but $\mathrm{C}_R = \phi$ because $x^4 >_{\{1,4\}} x^3 >_{\{3,4\}} x^2 >_{\{1,2,3\}} x^4$ and $x^4 >_{\{3,4\}} z_{12}$, where all these coalitions are decisive.

We now turn to some of the qualitative features of the various core concepts. Recall that

$$\mathrm{C}_X = \bigcap_{K^d} P_c(X);$$

that is, the usual notion of the core is characterized by the intersection of the Pareto sets for all the decisive coalitions in K^d.

THEOREM 5.

(1) $\mathrm{C}_Q = \bigcap_{K^d} P_c(Q_c)$;

(2) $\mathbf{C}_I = \{x \in Q \mid x \in \bigcap_{K^d} P_c[Q_c(x)]\}$, *and*

(3) $\mathbf{C}_R \supseteq \bigcap_{K^d} P_c(Q_c)$;

but the converse is false.

Proof

1. $x \in \bigcap P_c(Q)$

$\rightarrow \sim$ [There exists $C \in K^d$, $\alpha \in A$ such that $z^*(\alpha) >_c x$]

$\rightarrow x \in \mathbf{C}_Q$

$\rightarrow y >_c x$ for no $y \in Q$, $C \in K^d$

$\rightarrow x \in P_c(Q)$, $\forall\, C \in K^d$.

2. $x \in \bigcap P_c[Q_c(x)]$

$\rightarrow \sim$ [There exists $C \in K^d$, $y \in Q_c(x)$ such that $y >_c x$]

$\rightarrow x \in \mathbf{C}_I$

$\rightarrow y >_c x$ for no $y \in Q_c(x)$, $C \in K^d$

$\rightarrow x \in P_c[Q_c(x)]$, $\forall\, C \in K^d$,

with the last implication following by $x \in Q_c(x)$, all $C \subseteq K$, all $x \in Q$.

3. $x \in \bigcap P_c(Q_c)$

$\rightarrow \sim$ [There exists $C \in K^d$, $\alpha \in A_c$ such that $z^*(\alpha) >_c x$]

$\rightarrow x \in \mathbf{C}_R$.

To check that the converse is generally false, consider Figure 6 and the associated discussion. QED

Thus, the core concepts $\mathbf{C}_Q$ and $\mathbf{C}_I$ have the same "intersecting Pareto sets" property as the usual core. ($\mathbf{C}_I$ has to be modified somewhat, since points other than x may be in $Q_c(x)$.) On the other hand, $\mathbf{C}_R$ may include elements not in

$$\bigcap_{K^d} P_c(Q_c),$$

since, in contrast to the sets Q and $Q_c(x)$, there is no guarantee that $x \in Q_c$ for all $C \in K^d$.

In Figure 6, we suppose $w_j = 1/5$ for all j: clearly, $\mathbf{C}_X = \phi$. Consider $C = \{3, 4, 5\} \in K^d$, and observe that $P_c(Q_c) = \{z_{45}, z_{43}, z_{53}, z_{35}, z_{34}, x^3, x^4, x^5\}$. Similarly, for $C' = \{1, 2, 3\} \in k^d$ we have $P_{c'}(Q_{c'}) = \{z_{12}, z_{23}, z_{32}, z_{21}, x^1, x^2, x^3\}$. Finally, consider $C'' = \{1, 2, 4\} \in K^d$ with $P_{c''}(Q_{c''}) = \{z_{14}, z_{42}, z_{24}, z_{12}, z_{21}, z_{41}, x^1, x^2, x^4\}$. Then $P_c(Q_c) \cap P_{c'}(Q_{c'}) \cap P_{c''}(Q_{c''}) = \phi$. However, it is straightforward to check that $\mathbf{C}_R = \{z_{12}, x^2\}$. This proves the final claim of Theorem 5. In addition, the example in Figure 6 reveals two further facts. First, $\mathbf{C}_R \neq \phi$ does not imply $\mathbf{C}_I \neq \phi$ (or, therefore, $\mathbf{C}_Q \neq \phi$). To see this, note that $z_{41} = (0, 0) \in Q_c(z_{12})$ and $z_{41} >_c z_{12}$; and $z_{32} \in Q_c(x^2)$ and $z_{32} >_c x^2$. (Notice that although z_{41} is preferred to both points in $\mathbf{C}_R$ by coalition C, $z_{41} \notin Q_c(x^2)$.) Second, minority governments can arise in equilibrium: both alternatives in $\mathbf{C}_R$ are implemented by $K(\alpha) \notin K^d$.

As in the proof of Theorem 1, define the set of issue-by-issue medians in X by

$$M = \{\mu \in X \mid \forall r = 1, \ldots, m, \sum_{L_1(r)} w_j \leq 1/2 \text{ and } \sum_{L_2(r)} w_j \leq 1/2\},$$

where $L_1(r) = \{j \in K \mid x_r^j < \mu_r\}$ and $L_2(r) = \{j \in K \mid x_r^j > \mu_r\}$. Recall that the usual definition of the core gives a "centrally located" prediction in that if, for example, u_j is separable for all $j \in K$, $\mathbf{C}_X \subseteq M$.

THEOREM 6. *(1) If Ω is such that u_j is separable for all $j \in K$, $\mathbf{C}_I(\Omega) \subseteq M$, and $\mathbf{C}_Q(\Omega) \neq \phi$ implies $\mathbf{C}_Q(\Omega) = \mathbf{C}_I(\Omega)$; (2) There exists Ω such that $\mathbf{C}_R(\Omega) \neq \phi$ and $\mathbf{C}_R(\Omega) \cap M = \phi$; and (3) For all $t = Q, I, R$ there exist Ω such that $\mathbf{C}_t(\Omega) \neq \phi$ and $P_c(X) \cap \mathbf{C}_t(\Omega) = \phi$, $\forall\, C \in K$.*

Proof:

1. If not, there exists $x \in \mathbf{C}_I$ such that $x \notin M$. Hence, there exists $r \in \{1, \ldots, m\}$ such that

$$\sum_{L_i(r)} w_j > 1/2,$$

for some $i \in \{1, 2\}$. Consider $y = (\mu_r, x_{-r}) \in X$, where μ_r is the rth component of some $\mu \in M$ such that $\mu_r = x^j_r$, some $j \in L_i(r)$. Therefore, by separable preferences and $x \in Q$, $j \in L_i(r)$ implies

$$y \in Q_{L_i(r)}(x).$$

Moreover, separable preferences imply

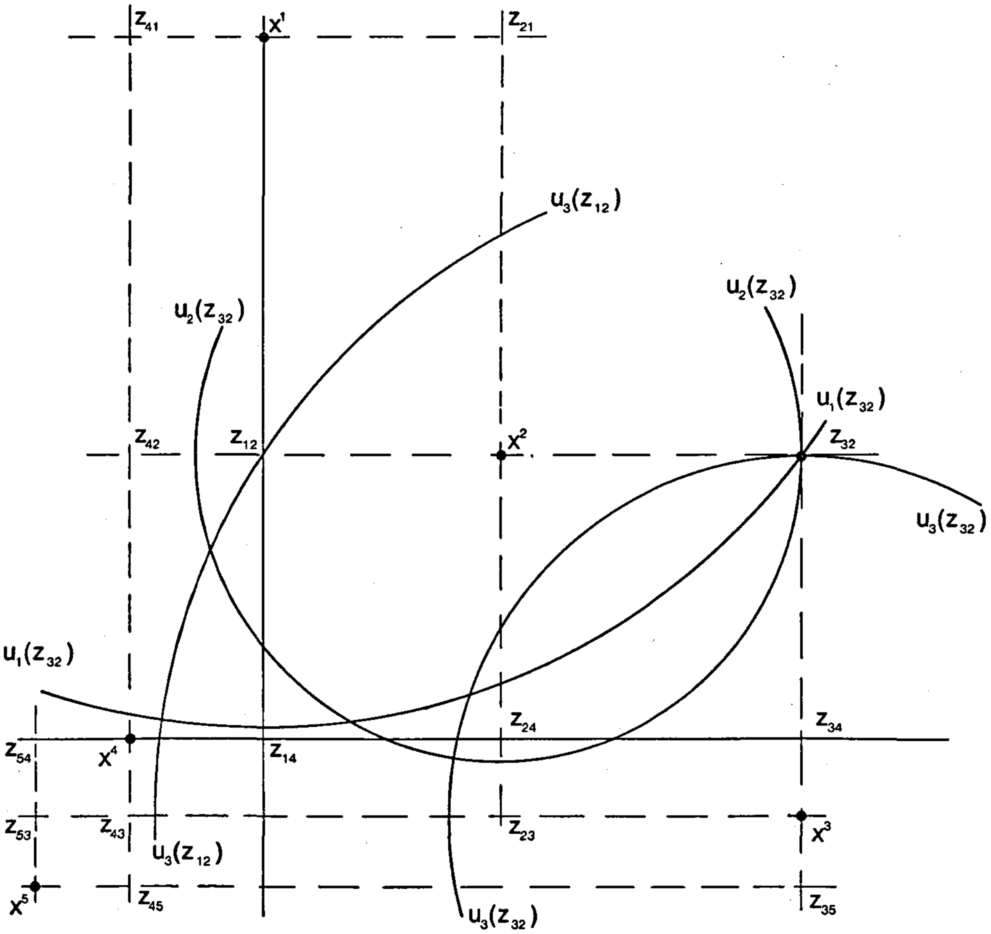
$$y >_{L_i(r)} x.$$

Since $L_i(r) \in K^d$, this contradicts $x \in \mathbf{C}_I$. Recalling $\mathbf{C}_Q \subseteq \mathbf{C}_I$, the argument also establishes the remaining claim of Theorem 6.1.

2. See Figure 6: $\mathbf{C}_Q = \mathbf{C}_I = \phi$, $\mathbf{C}_R = \{z_{12}, x^2\}$, and $M = \{z_{14}\}$.
3. Let $X \subseteq \mathbf{R}^3$ and $K = \{1, 2, 3\}$. Suppose all preferences Euclidean, $x^j = e_j$, and $w_j = 1/3$ for each $j \in K$. Clearly, $\mathbf{C}_X = \phi$. However, it is easy to confirm $\mathbf{C}_Q = \mathbf{C}_R = \{(0, 0, 0)\} \notin P_c(X)$, all $C \in K$.

QED

Figure 6. $\mathbf{C}_R$ Is Noncentrist and Nonempty; $\mathbf{C}_Q$ and $\mathbf{C}_I$ Are Empty



z_{41}
x^1
z_{21}
$u_3(z_{12})$
$u_2(z_{32})$
$u_2(z_{32})$
$u_1(z_{32})$
z_{42}
z_{12}
x^2
z_{32}
$u_3(z_{32})$
$u_1(z_{32})$
z_{24}
z_{34}
z_{54}
x^4
z_{14}
z_{53}
z_{43}
$u_3(z_{12})$
z_{23}
x^3
x^5
z_{45}
$u_3(z_{32})$
z_{35}

Note: $w_j = 1/5$ for all j; $M = \{z_{14}\}$; $\mathbf{C}_Q = \mathbf{C}_I = \phi$, and $\mathbf{C}_R = \{x_{12}, x^2\}$.

Condition 1 shows that with separable preferences, the intermediate portfolio core (and hence the portfolio core) is necessarily a subset of the set of issue-by-issue medians, which is true of the core C_X as well. In addition, with separable preferences the intermediate portfolio core is an "extension" of the portfolio core C_Q in that when C_Q is nonempty it coincides with C_I. On the other hand, condition 2 shows that C_R need not include *any* issue-by-issue medians; however, condition 1 and $C_I \subseteq C_R$ imply that such a situation only occurs when $C_I = \phi$. Finally, condition 3 implies that none of the core concepts predict that stable outcomes are necessarily Pareto-efficient in the set of alternatives X.

Conclusion

The core C_X is a natural solution concept for predicting outcomes from any weighted voting game over some set of alternatives X. Unfortunately, C_X is typically empty. In such circumstances, it is often argued that outcomes are as much a function of institutional detail other than the simple specification of the voting rule as of individual preferences. Institutions restrict the sets of alternatives that may come against each other in any vote, thus reducing the possibility of voting cycles. Given the appeal of the core as the basic solution concept, therefore, and given an institutional structure, it is appropriate to look for outcomes lying in the core of the restricted set of alternatives. Here, we do exactly this in the context of government formation and policy choice in multiparty legislatures.

In the model a winning coalition in the legislature cannot implement any policy it wishes. Policies are implemented by the winning coalition allocating responsibility (portfolios) for particular components of policy to particular parties. These parties —collectively referred to as the *government*—noncooperatively choose those aspects of policy within their portfolios to maximize their final payoffs. The idea is that while coalitions might freely form to overturn or create governments, there is no available internal mechanism by which a coalition can ensure that its members will make particular decisions. Were this not the case, the allocation of portfolios would be redundant. However, there are now three possible definitions of the core for the voting game played under this institutional structure.

The first definition implies that a winning coalition can allocate portfolios to parties not themselves in the coalition (C_Q). Whether or not such a possibility is reasonable will depend on the opportunity cost of the (noncoalition) parties being offered portfolios. If this cost is sufficiently low, such parties will accept the portfolio and the definition is reasonable; but if the cost is high enough, the definition is not so reasonable. An alternative definition of the core insists that only coalition members can hold portfolios (C_R). In this case, equilibrium outcomes (i.e., nonempty core) exist more frequently than under the first definition. However, whereas any outcome in C_Q is centrally located (given that C_Q is nonempty), the same is not true of the alternative. Thus, there can exist policies in C_R against which there is a winning coalition with a more preferred policy, z; but z is such that the coalition cannot implement z by relying solely on its own members. On the other hand, it is precisely in such circumstances that C_Q might be empty while C_R is not.

A final possible definition considered here lies between C_Q and C_R. In this definition (C_I) decisive coalitions are unable to compel nonmembers to accept new portfolios. However, they are capable of taking portfolios away from nonmembers and allocating them to coalition parties; and they are also able to insist that nonmembers who hold portfolios continue to hold them. Thus, as with the restricted portfolio core, C_R, decisive coalitions can only replace existing government parties with its own constituent members; but, unlike the case with C_R, decisive coalitions are free to leave parties in the original government holding their portfolio whether they are coalition members or not. Evidently, to investigate which of these concepts is the most compelling requires an explicit description of how coalitions form, and so on; that is, the necessary model is noncooperative (e.g., sequential bargaining).

Three additional substantive features of the model are worth noting. The first is that the model predicts both the stable governmental policies and the makeup of the government supporting, or rather generating, such policies. This is in contrast to the models described in the opening of the paper, where the issues of government policy and portfolio assignment were analyzed independently. Second, the model supports the existence of "minority" governments, insofar as the government is defined to be the set of parties holding nonempty portfolios. However, minority governments need the tacit support of some winning coalition, and this will be forthcoming only to the extent that the policy outcome implemented by the government is at least as good as any winning coalition could achieve by replacing the government. Third, although we assumed throughout that feasible portfolio allocations could be associated with the usual basis for the policy space, there is no a priori reason why this need be so. Empirically, the definitions of various regimes of responsibility in governments have changed; hitherto disjoint ministries have been combined; new portfolios spanning the jurisdictions of previous agencies have been created; and so on and so forth. Theorem 4 demonstrates that in some circumstances a nonempty core (C_Q) can exist relative to some basis to the space, if not relative to the usual basis. This fact suggests a richer model of portfolio allocation in which the basis on which portfolios themselves are to be defined is endogenous.

Finally, it is worth discussing how our theory and results bear on the notion of structure-induced equilibrium, introduced by Shepsle (1979). We can think of portfolio allocations as corresponding to committee jurisdiction assignments, where the family of potential committees coincides with the set of legislative parties. So in the legislative context the model is one in which committee assignments and jurisdictions are simultaneously allocated. The theory of structure-induced equilibrium begins with a given allocation α and explores the features of the set $Q(\alpha)$. In particular, while C_X may be empty, $Q(\alpha)$ is nonempty and consists of the policy outcomes that are stable, given committee assignments and jurisdictional structure. Outcomes in the core, say C_Q, are then outcomes supported by stable committee assignments. In other words, if $z^*(\alpha) \in C_Q$ for some allocation α, α describes a stable committee assignment. However, the fact that C_Q can be empty implies that no stable committee assignments need exist. Consequently, by endogenizing committee assignments and jurisdictions, the stability of structure-induced outcomes becomes problematic. On the other hand, Theorems 5 and 6 provide characterizations for structure-induced outcomes when stable committee assignments exist.

Note

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THE AGONY OF POLITICS: THE NIETZSCHEAN ROOTS OF FOUCAULT'S THOUGHT

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The politics of Michel Foucault remain controversial, mainly because they remain ambiguous. His critics charge that the ambiguity is symptomatic of a theory that fosters moral abandon and political impotence. This predicament is frequently attributed to Nietzsche's influence. I outline the critics' charges and investigate to what extent Foucault's thought evidences Nietzschean roots. Specifically, I examine Nietzsche's glorification of struggle and seek to demonstrate that a valorization of struggle constitutes the core of Foucault's political thought.

Critiques and apologies of the work of Michel Foucault are remarkably uniform in character. Apologists generally attempt to convey the sense of liberation achieved through Foucault's deconstruction of the subject, of history, and of the modern mechanisms of *power/knowledge.* Foucault's readers feel challenged to free themselves from constraining and cooptive metadiscourses, such as (the early or evolved versions of) Hegelianism, Marxism, liberalism, and Freudianism.[1] One is not to take part in the anthropologization of thought, which, asserted Foucault, has dominated Western minds since Kant. One is to refuse to define humanity in metaphysical, historical, economic, juridical, or sexual terms. Indeed, humanity must not be defined at all. The modern discourse of humanity, of its truth and its ultimate liberation, must be left behind. In large parts, this resistance to anthropological essentialism constitutes what might be called Foucaultian liberation. For in the modern world the most insidious forms of power are shown to be productive forces engaged in the subjectification of their participant victims. Modern power not only restricts, it incites—and does so by means of administering over the self-definition of its subjects. However, because social and political life entails an ongoing imposition of definition, an escape from subjectification is impossible. Resistance to its forces remains the only alternative.

Freedom, Foucault maintained, must be understood as resistance to the impositions of power, as the antimatter of power. Freedom is less a state of being, characterized traditionally by the absence of repression, domination, and exploitation than a kind of activity in the nexus of opposing forces. Resistance cannot exist without power any more than power can exist without resistance. Freedom emerges as the product of this symbiotic relationship. What glimpses of freedom are available to us in local and specific acts of insurrection and insubordination, however, are to be attained only after a sobering reevaluation of the potency, productivity, and limitless domain of power/knowledge. The alternative to such sobriety is to remain in thrall to the opiate belief in ab-

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solute emancipation. Then, in the delusory struggle for such unlimited freedom, one would unwittingly reinforce one's attachment to the stratagems of power/knowledge, as the frenzied movements of a newly captured fly only serve to entangle it hopelessly in the spider's web. Recognition of one's entanglement in the webs of power/knowledge, Foucault's defenders maintain, is a form of liberation in itself.[2]

Critics of Foucault lament the atmosphere of apathy and impotence which pervades a theory founded on the inescapability of power/knowledge relations. Those who, à la Foucault, attempt to theorize without a subject, without a moral or metaphysical ground, and without a definite goal are (so it is claimed) necessarily without a politics. The ceaseless Foucaultian flight from the ubiquitous powers of normalization precludes any defensible normative position. Hopelessness is the accompaniment to lost innocence. Nihilism is the only alternative for those who spurn all forms of essentialism. The critics' lament constitutes an accusation of moral and political abandon, summarized in Habermas' question to Foucault: If power is inescapable, why fight? (1966, 7).[3] While ostensibly offering his readers a "tool kit" whose contents might be employed to deconstruct the apparatuses of modern power, Foucault, according to the charge, at best produced a justification for modern Luddism. At worst, one is denied any reason or moral ground to take up these weapons of resistance. And by erasing the face of the enemy, thus making him both anonymous and less antagonistic, the targets of deconstructive tinkering remain unidentifiable. Within such a spiderless web of power, acts of resistance hardly seem sustainable. Moreover, in refusing to offer a vision of a better future, one in which most weapons might be replaced in their box, a victory having been won, Foucault fatally undercut the impetus to struggle.

The charge against Foucault, in short, is that leveled against postmodernist writing in general. Foucault's understanding of power as ubiquitous, inescapable, stemming from below, and productive of our very identity, is, I believe, the key concept of postmodern political thought. Postmodernism signifies a rejection of the faith modern humanity still had in itself as transcendent, as something that was not as malleable as its technological advances had shown the rest of nature to be. Thus, postmodern politics, perhaps an oxymoron, is seen to be an exercise in futility, a nihilistic project without a goal. For politics, since the time of Aristotle, was thought to be that which allowed humanity the fullest realization of its own nature. Since the rise of liberalism, politics was thought to be that which safeguarded humanity its greatest good, freedom. Modern power, Foucault insisted, has undermined both projects: there is no human nature and there is no absolute freedom from which power is absent. According to Foucault, there is no realm of freedom in which we may escape power to assert our nature: we might change our positions on the web, but there is no jumping off. To the extent that Foucault is postmodern, then, he is seen to be apolitical if indeed not antipolitical. In the words of Todd Gittlin, "Post-modernism is anticipatory shell shock. It's as if the Bomb has already fallen. . . . Post-modernist writing confesses (or celebrates) helplessness. Make the most of stagnation, it says; give up gracefully. That is perhaps its defining break from modernism, which was, whatever its subversions, a series of declarations of faith" (*New York Times Book Review* 1988). Modernism was a declaration of faith in humanity as a transcendent subject that might one day build for itself a realm of freedom unsullied by the corruption of power. Foucault's critics, by this characterization, appear wary of Foucault's outright rejection of the modernist project. A call to faith—albeit one schooled in the history of the failures and horrors of modern times—escapes their lips.

I do not wish to lend my voice to the modernist chorus of lament. At the same time, my purpose is not simply to defend Foucault with his own words. Rather, I shall investigate the dynamics that underlie much of Foucault's work in an attempt to discover whether a defense against his critics' claims is to be found submerged in his writings. The argument is not that Foucault has resolved the problems of modern politics, voiding his critics' concerns, but that Foucault's work allows a tentative formulation of how we might best address these concerns. To this end I must enter into a discussion of Friedrich Nietzsche, who, in effect, marked the beginning of the end of modernism. I shall illustrate Nietzsche's glorification of struggle in the face of nihilism and attempt to demonstrate that much of Foucault's work finds its nourishment and justification in such a Nietzschean agon. My aim, in short, is to demonstrate how the Nietzschean roots of Foucault's thought lend it their support.[4]

Foucault's indebtedness to Nietzsche is generally recognized if seldom analyzed. But for most critics, such as Charles Taylor, Foucault's Nietzscheanism is what gets him into the sort of theoretical and practical predicaments that allow no creative solutions (Taylor 1984). Similarly Jürgen Habermas censures Foucault for the *aporias* resulting from his Nietzschean "irrationalism" and "militant perspectivism" (Habermas 1986, 2, 5). Indeed, Foucault stated that what distinguished his own from the Habermasian project was precisely his Nietzschean predilection (Foucault 1984a, 250). The problem, according to Foucault's critics, is his unwillingness to speak in terms of justice and its imperatives; and this unwillingness, Foucault himself admitted, may be attributed to Nietzsche (Foucault 1974).

His Nietzschean roots, then, bring Foucault to an apparent moral and political impasse. Nietzsche himself, as will be demonstrated, offered a means of passage, albeit a tragic one. I suggest that Foucault began to blaze his own trail. While the markings are often unclear and the course unfinished, its direction is nonetheless implied.

Nietzsche and the Heroic

God had died in Nietzsche's world. Nietzsche did not claim responsibility for the killing, but he was enthusiastic about celebrating the wake. Yet the modern world was also inhospitable to heroes, the half-gods and godlike men who might redeem life through their greatness. This was Nietzsche's concern. For nihilism, the bane of modern life, was just such a denial of the heroic, the denial of all greatness, the depreciation of all striving. The Nietzschean project, in short, was to establish a passion for greatness in a world without gods.

Thus, Nietzsche was concerned—indeed, one might say obsessed—with the fate of the hero. He recounted the decline and fall of tragic heroism. He described the ascent of the antiheroic type who crept onto the stage with Socrates and consummated its role in Christianity. He lamented the absence of heroism in the modern world while rejecting the romantic attempt to resuscitate it. And he presented a hero for the times, one whose strengths of character would prove capable of establishing a golden age of order and growth—if not within society at large at least within the ranks of higher man. The *overman* is proposed as the hero of a nihilistic age. Like his forerunners, he bears his own standards of morality and reason, and attempts to vanquish the hitherto reigning traditions and values. Unlike his forerunners, the overman makes no claim to divine sanction for his deeds. He refuses to be armed by the gods, as was the boon claimed by the greatest heroes of the ancient poets. The overman is the hero of an atheistic and morally destitute world; he presents the paradox of the avid pursuit of greatness when no transcendental standards exist. He must embody his own justification.

For Nietzsche struggle is the essence of the heroic. The hero is the *agonal* spirit incarnate. He finds in struggle both his means and his end. He desires his friends to be his fiercest opponents. He bears a "spiritualized enmity" toward himself, a soul "rich in contradictions" that "does not relax, does not long for peace" (1968a, 44). Strife is not merely tolerated, it is welcomed. The hero's life is the story of battles fought and obstacles overcome; his glory a measure of the dangers involved and the courage displayed. He is an individual, a mortal plagued by the limits of the self, which he continually attempts to transgress. The heroic ideal is nothing less than apotheosis. Thus, the hubris that predestines him to a tragic fate.

Nietzsche's earliest philosophical works already mark his captivation by the concept of the heroic. *The Birth of Tragedy*, like many of the unpublished writings preceding it, is essentially an encomium and account of the origins of the ancient tragic hero of the Greek playwrights (most notably Sophocles and Aeschylus). The dismay accompanying his downfall at the hands of Euripides and the rationalistic Socrates is matched with hopes for the resuscitation of a tragic culture in Germany through music (most notably Wagner's). Nietzsche saw himself as the prophet of a new age of "dragon-slayers" who would display "a heroic penchant for the tremendous" (1967a, 112).

The *Untimely Meditations* continue the attempt to spur the growth of heroic individuals through cultural development. The underlying theme of its four essays is the desirability of creating a heroic culture to replace the modern pseudoculture. The good life is held to be one spent creating and maintaining culture, which is defined as the favorable environment for the propagation and maintenance of great men. The essays are marked by a voluble hopefulness that the creation of such an environment is possible in Europe. The means to its establishment is the emergence of a relatively small number of heroic figures: "Satiate your soul with Plutarch and when you believe in his heroes dare at the same time to believe in yourself. With a hundred such men—raised in this unmodern way, that is to say become mature and accustomed to the heroic—the whole noisy sham-culture of our age could now be silenced for ever" (Nietzsche 1983, 95).

Human, All Too Human, Nietzsche's next publication, marked a distinct break in his assessment of culture. The book does not establish a positivist worldview, as is often asserted, but it does supply a rationalistic or scientific critique of all forms of romanticism, including Nietzsche's own. He voiced suspicion of his earlier enthusiasm and engaged in polemic against his mentors. A growing skepticism is pitted against his former tendency toward naive idealism and the romantic adulation of great men. Indeed the book, as Nietzsche later characterized it, was an attempt to put "the hero" on ice (1967b, 284). Nonetheless, Nietzsche's heroic form of philosophizing has not been altered. Only the object of his accolades has changed. The hard-nosed skeptic is submitted as the new model for emulation. His strength and courage is such that he no longer needs the intoxicants and palliatives of romantic culture. Romanticism, particularly Wagner's, is now held to be antiheroic, a doctrine not of fearless struggle but of indulgence. Nietzsche's hopes for a cultural renaissance were dashed by the discovery that the romantics' penchant for heroism was not a stimulant to self-development but a means of avoiding it, a surrogate for change.

Romanticism, at best, was a vicarious heroism. Nietzsche had deceived himself into believing that he had heard a clarion call when in fact it was but a sentimental dirge. Romanticism was rejected because it lacked the fundamental quality it was originally believed to incarnate, *the will to struggle.*

No longer actuated by hopes of the total revivification of culture, Nietzsche nonetheless retained his own heroic status. His deed was to be less sensational but nonetheless one befitting his self-understanding. Toward the end of *Human, All Too Human* Nietzsche played his own minstrel: "*This, too, is worthy of a hero.* —Here is a hero who has done nothing but shake the tree as soon as the fruit was ripe. Do you think this too little? Then take a look at the tree he shook" (1986, 393; original emphasis). The timely attack on all forms of romantic decadence and moral idealism, carried out with discipline and rigor, would yield a worthy harvest even though such unglorious labor is contrary to the hero's romanticized image. A passage of *Daybreak,* Nietzsche's next book, would reassert the status of the iconoclast: "To do things of the vilest odor of which one hardly ventures even to speak but which are useful and necessary—this too is heroic. The Greeks were not ashamed to include among the great labors of Heracles the cleansing of a stable" (Nietzsche 1982, 185). Nietzsche's future genealogical studies of morality and religion would prove to be his own Heraclean—albeit meticulous—task of antisepsis.

Much of the remainder of Nietzsche's corpus exhibits a similar tendency. Former positions are criticized and contradicted, regarded not as platforms to be maintained but as steps to be overcome en route to the incarnation of an ever-changing image of the hero. His so-called positivistic writings are subject to his own censure just as his romantic writings had been. This is in keeping with Nietzsche's self-image. For the hero is not so much characterized by a particular set of beliefs as by the willingness to attack his own convictions and prejudices. His standards of virtue evolve along with him. "Only he who changes remains akin to me," Nietzsche would insist (1972, 204). The striving for excellence is tantamount to endemic change in the individual who is always in competition with former selves.

Nietzsche spoke of a "*refined heroism* which disdains to offer itself to the veneration of the great masses, as his coarser brother does, and tends to go silently through the world and out of the world" (1986, 134; original emphasis). The criteria for heroism were spiritualized. Nietzsche was most anxious to distance himself from those coarse relatives who also were concerned with the heoric. He repudiated all forms of "hero worship," specifically Carlyle's (Nietzsche 1967b, 261). The "romantic prostration before 'genius' and the 'hero' " was recognized to be an integral part of contemporary culture and was considered antithetical to his own efforts. Hero worship, or "unconditional homage to people" was thought dangerous and quite simply "ludicrous" (Nietzsche 1982, 103). "It has always been the greatest fatality for a culture when men have been worshipped," Nietzsche remarked (1986, 259). The cult of the hero—the great man or genius—was condemned as a throwback to theistic times, "an echo of this reverance for gods and princes" (p. 168).[5] Nietzsche was concerned that he not be identified with those who erected and worshipped golden calves in the place of forgotten gods. Among the idols he hoped to destroy were all those that bore the stamp of divine mimicry. This was not to reject his devotion to the creation of the conditions for the emergence of great men or his belief that the goal of culture was contained in its highest exemplars. The truly heroic culture, however, produces great men not so much to bow down before them as to step beyond them. The heroic individual must serve as a stimulation to further growth and the production of a still higher type: "A people is a detour of nature to get to six or seven great men. —Yes: and then to get around them" (Nietzsche 1972, 81). Hence the romantic worshippers of the heroic were identified as enemies, not allies, and were treated accordingly.

In part, what distinguishes the Nietzschean hero from his coarser brother is that the former never finishes his labors. Because his victories are self-overcomings, his opposition is never vanquished. The release from his labors could be found only in the release from consciousness. For consciousness is always consciousness of limitations. His deeds of self-overcoming and his words advertising these victories serve as much to mark his boundaries as his achievement. Herodotus' lament might come to his lips: "Of all the sorrows that afflict mankind, the bitterest is this, that one should have consciousness of much, but control over nothing." The classical hero fought against this fate in his struggles against Fortuna. To transgress his limits the hero must win the favor of the gods or else battle with them, asserting his will over their divinity. But the goddess of chance is never conquered, only temporarily subdued. The heroic penchant for adventure is the tempting of Fortuna into doing battle again and again. The classical hero struggled with Fortuna primarily in war and on the high seas. Nietzsche's heroes are "adventurers and circumnavigators of that inner world called 'man' " (1986, 10). The battlefield is within the self, and the corpus of knowledge supplies all the mysteries and intrigue of uncharted waters.

The hope is that a higher age "will carry heroism into the search for knowledge. . . . For believe me: the secret for harvesting from existence the greatest fruitfulness and the greatest enjoyment is—to *live dangerously!* Build your cities on the slopes of Vesuvius! Send your ships into uncharted seas! Live at war with your peers and yourselves!" (1974, 228; original emphasis). The "heroism of knowledge" of which Nietzsche spoke requires all the courage possessed by its classical forerunner; but it has "at length grown subtle, spiritual, intellectual" (1969, 313). It is from the greatest dangers that the greatest glory is won, asserts Thucydides' Pericles. The classical hero was prone to rash and reckless deeds. Nietzsche's higher man also seeks glory, albeit a self-administered kind. The greatest dangers of the modern world, however, are not to be found at the end of a spear but rather with pen in hand and during solitary walks. The free spirit consequently "tends to moderation" in action, for his energies are channeled into "the promotion of spiritual objectives" (Nietzsche 1986, 169). The modern hero finds his nemesis in himself as he totters on the abyss of nihilism. His payment for glory is not in the coin of sweat and blood, but in isolation, anguish, spiritual torment, and perhaps ultimately the loss of sanity. Valor's task is to seek out this strife and walk this precipice.

The quest for knowledge, wrote Nietzsche, demands a "heroic mood"; and he characterized his own project as requiring a "heroic way of thinking" (1920–29, 11:13, 1975–84, 13 July 1882). The will to struggle, however, is not the product of a primordial yearning for knowledge. Just the opposite is true. All genuine striving for truth is the product of "the pathos of struggle" (1920–29, 6:17). The heroic disposition is primary. The quest for knowledge is merely the means employed by the modern hero to exhaust his energies. Not truth but the struggle for truth claims his heart. His world is one of adventure and mystery on an anchorless ship of discovery. Truth without opposition—a "tyranny of the true"—even if possible would be rejected as "boring, powerless and tasteless" (Nietzsche 1982, 206). Above all, the pathos of struggle must be maintained and cultivated.

The hero of knowledge must suffer for his booty, paying for his victories with the loss of his former beliefs and identity. He glories in the necessarily endless and ultimately futile quest for truth and proclaims the world as will to power, as the pathos of struggle between competing perspectives. He anticipates his opponents' accusations of self-contradiction as he confidently asserts the impossibility of transcendent truth, and replies, "Granted this too is only interpretation—and you will be eager enough to raise this objection?—well, so much the better" (Nietzsche 1972, 34). His response is made in the only manner consistent with his character—as a challenge.

War and oceanic odysseys were not the penchant of this erstwhile Basel professor. Nor was politics. Politics or statecraft (Nietzsche considered them synonymous) ought be left to the statesmen, of whom Nietzsche held no high opinion: "Political and economic affairs are not worthy of being the enforced concern of society's most gifted spirits: such a wasteful use of the spirit is at bottom worse than having none at all" (1982, 107; see also 1983, 181). Included among Nietzsche's "Ten Commandments for Free Spirits" is the proscription, *You shall not practice politics* (1920–29, 9:365). Indeed, Nietzsche intentionally made his published work politically obnoxious lest his sole public role as a writer constitute an infraction of his edict: "I write in such a way that neither the mob, nor the populi, nor the parties of any kind want to read me. . . . Neither usefully nor pleasantly—to the trio I have named" (1986, 327–28). To be antipolitical, as Nietzsche considered himself (1967b, 225) is the mark of individuality.

The Nietzschean hero, in distinction to his classical forerunners, spurns the intrigues and masquerades of the stage of worldly affairs to face a more demanding audience within himself. The criteria of his judgments are primarily aesthetic. The hero of knowledge ultimately must be evaluated using artistic standards. His final words, *Ecce homo* (Behold the man) are both the proclamation of his heroic individuality and the advertising of a tragic masterpiece.

For Nietzsche existence is justifiable only as an aesthetic phenomenon (1967a, 52); that is to say, one's life (and in particular a life devoted to the pursuit of knowledge) becomes "bearable" only through its transformation into an aesthetic experience (1974, 164). The goal is to stylize one's life, to make of oneself a work of art by means of best arranging one's numerous "undersouls." Nietzsche attacked the doctrine of "soul atomism" and hypothesized that the individual was a composite of many souls, a "social structure of the drives and emotions" whose hostilities and resolutions produced consciousness and defined one's character (1972, 25; see also 1968b, 270). To have great and conflicting passions and to exhaust the power produced by this inner agonism is the mark of grand style, great art, and tragic-heroic men.

But such manipulations of the soul entail the willingness to sacrifice stable identity, to change and become other. For destruction and creation are inseparable within the dynamic of self-overcoming: "You do not have the courage to burn up and perish: and so you never know that which is new. What today are my wings, power, clothes, and color, shall be my ashes tomorrow" (Nietzsche 1920–29, 14:13). The project to make of one's life a work of art resounds with the imperative to become a Phoenix. "All great things bring about their own destruction through an act of self-overcoming," Nietzsche wrote, foreseeing and lauding his own destiny (1967c, 161).

The hero has the fate of Tantalus whose reach is insufficient and whose efforts are unending. For the fruit of struggle is un-attainable: he is a mortal who seeks immortality, a socially constructed entity that seeks absolute independence, a man who desires to be a god. But as he reaches for what he cannot grasp, he also grows in power and therefore welcomes the temptation to overstep his limits. That his fate is preordained, that he is doomed to perish through his own excesses is a matter of indifference. Or rather, the opportunity to perish in pursuit of a lofty goal beyond the reach of mortal man is deemed a blessing. Nietzsche's definition follows: "Heroism—that is the predilection of a man who strives for a goal, which reckoned with, he himself no longer comes into consideration. Heroism is the good will to self-destruction" (1920–29, 14:52–53). Self-sacrifice is not in itself desired; it is simply the byproduct of a struggle to attain a goal that is forever out of reach. The hero becomes indifferent to his pain and survival in the pursuit of an ideal.

The hero must justify his existence after the fact by setting himself a noble goal; by making of himself something that redeems life, namely, an aesthetic project; and by risking and often losing his life as a result. Divine ends and mortal means ensure a tragic fate. But he gladly embraces such a destiny. Nietzsche's words might serve as the hero's motto: "I know of no better aim of life than that of perishing *animae magnae prodigus* [careless of life] in pursuit of the great and the impossible" (1983, 112). The impossibility of a final victory does not dissuade him from the task because his defining characteristic is not his having a great goal but his having great vigor. The hero is a "monster of energy, who demands a monster of a task" in order to expend himself. His actuating force is the will to struggle, "and the wretched spiritual game of goals and intentions and motives is only a foreground—even though weak eyes may take them for the matter itself" (Nietzsche 1968b, 518).[6]

Nietzschean Agon and Foucaultian Politics

"Struggle is the perpetual food of the soul, and [the soul] knows well enough how to exact the sweetness from it," Nietzsche wrote in his notebook (1920–29, 1:183). Foucault, I contend, laid claim to this same nourishment; and the will to struggle—so prominent in Nietzsche's thought—was also Foucault's actuation. It structures his theory and informs his practice. If we are to accept (perhaps with some irony) Edward Said's assessment of Foucault as "the greatest of Nietzsche's modern disciples" (Said 1984, 1) it must be based on Foucault's inherited agonism.

Foucault paid much homage to Nietzsche and claimed to be, if not following in his footsteps, at least making use of the light of embers still aglow on the path Nietzsche burnt. As early as *Folie et déraison*, Foucault situated his work "under the sun of Nietzsche's great search" (1961, v). Nietzsche, wrote Foucault in *The Order of Things*, set aflame all the elements of nineteenth-century thought. With the announcement of the death of God, Nietzsche was already forecasting the death of Man, God's murderer. At the same time, Nietzsche broke and scattered "the great continuous chain of History," allowing neither Man nor History to fill the void of nihilism left by the death of God. "It was Nietzsche," said Foucault, "who burned for us, even before we were born, the intermingled promises of the dialectic and anthropology" (1970, 203). From these ashes, Foucault speculated, might grow the seeds of future thought: "In this, Nietzsche, offering this future to us as both promise and task, marks the threshold beyond which contemporary philosophy can begin thinking again. . . . For this void does not create a deficiency; it does not constitute a lacuna that must be filled. It is nothing more, and nothing less, than the unfolding of a space in which it is once more possible to think" (p. 342; see also 1977, 38).

Foucault proposed that his subsequent studies be seen as attempts to "constitute the 'political economy' of the will to knowledge" (1978a, 73). The term *will to knowledge*, as well as the genealogical method used to track it, was taken directly from Nietzsche. In Foucault's "Nietzsche, Genealogy, History," originally published in 1971, one finds amid the numerous references to Nietzsche's corpus all the tools and concepts later adopted by Foucault in *Discipline and Punish* and *The History of Sexuality*. All the trademarks of Foucault's methodology and concerns—the nature of genealogical study; the violence of (the origins of) truth; the cruelty, malice, and passion of the will to knowledge; and the sacrifice and self-sacrifice of the subject in the endless deployment of the will to truth—are documented as Nietzche's own. By the midseventies Foucault had placed his own archaeological method in the service of genealogy, whose purpose was to establish "a historical knowledge of struggles and to make use of this knowledge tactically today" (Foucault 1980, 83, 85). Were he willing to carry Nietzsche's banner (something he felt to be "pretentious"), Foucault announced, "he would use 'the genealogy of morals' as the general title for what [he was] doing" (p. 53).

With regard to his later work, Foucault was ready to admit that "it was Nietzsche who specified the power relation as the general form, shall we say, of philosophical discourse" (p. 53). It was also Nietzsche who construed the political question in terms of knowledge and power: "The political question, to sum up, is not error, illusion, alienated consciousness or ideology; it is truth itself. Hence the importance of Nietzsche" (p. 133). The proposition that truth is produced in the struggles and wars that amalgamate it with power and that at the basis of power relations lies the hostile engagement of forces, Foucault called, for convenience's sake, "Nietzsche's hypothesis" (p. 93).

A final conjunction: Foucault's writings concerning sexuality circumscribe the notion of creating the self as a work of art. This concern preoccupied Foucault in his latest works and was an endorsement of the Nietzschean project. In part, Foucault had shifted emphasis from genealogy to the equally Nietzschean endeavor of exploring the recreation of the self whose deconstruction was the genealogical task. The aestheticization of life rather than its moralization would allow for individual autonomy without sacrificing creativity to norms. Foucault's criticism of Sartre clarifies his own position: "Sartre avoids the idea of the self as something which is given to us, but through the moral notion of authenticity, he turns back to the idea that we have to be ourselves—to be truly our true self. I think the only acceptable practical consequence of what Sartre has said is to link his theoretical insight to the practice of creativity—and not of authenticity. From the idea that the self is not given to us, I think that there is only one practical consequence: we have to create ourselves as a work of art. . . . My view is much closer to Nietzsche's than to Sartre's" (1983b, 237; see also 1984b, 10–11).[7] Because humanity is now seen to have no stable identity, no intrinsic nature waiting to be realized, Foucault rejected moralistic discourse focusing on norms and standards. Humanity must create itself socially, culturally and historically. Like Nietzsche, Foucault understood that the alternative to passive nihilism entailed an artistic perspective.

This cursory examination of Foucault's Nietzschean roots might best be concluded with what amounts to Foucault's most explicit pledge of allegiance. In an interview conducted a few weeks before his death, Foucault responded to the question whether his link to Nietzsche did not constitute the source of the misunderstandings surrounding his work: "Do you mean to say that my fundamental Nietzscheanism might be at the origin of different misunderstandings? . . . I can only respond by saying that I am simply Nietzschean, and I try to see, on a number of points, and to the extent that it is possible, with the aid of Nietzsche's texts—but also with anti-Nietzschean theses (which are nevertheless Nietzschean!)—what can be done in this or that domain. I'm not looking for anything else but I'm really searching for that" (1985a, 9).

Foucault's extensive borrowing from Nietzsche's arsenal for his genealogical forays and exploratory reconstructions tempts one to search in Nietzsche for the answers to those questions Foucault seemed to evade. Yet such an investigation should not be couched in terms of Foucault's fidelity to Nietzsche. Foucault, it would seem, took to heart Zarathustra's words to his followers that "one repays a teacher badly if one remains only a pupil. And why, then, should you not pluck at my laurels?" (Nietzsche 1969, 103). As Foucault himself said, "The only valid tribute to thought such as Nietzsche's is precisely to use it, to deform it, to make it groan and protest. And if commentators then say that I am being faithful or unfaithful to Nietzsche, that is of absolutely no interest" (1980, 53, 54).[8] To be a Nietzschean genealogist is, to use one of Nietzsche's terms, a *contradictio in adjecto.* For the genealogist has no constants, not even the master's discoveries and methods. Still, Foucault did place himself under Nietzsche's sun, and his work gives evidence of this stimulation. The *aporias* in which Foucault is said to find himself, namely, the seeming impossibility of justifying struggle without transcendental standards in a faithless (some might add *hopeless*) age were already illuminated by his mentor. Nietzsche's solution, as we have seen, was an apolitical, tragic heroism, a philosophical and aesthetic life of continual self-overcoming. Foucault formulated its political analogue.

Politics, as generally described by Foucault, is the theatre of war and battle, tactics and strategies. Politics, he stated, simply is war carried out by other means.[9] Yet despite his reversal of the Clausewitzian thesis, Foucault refused to draw any specific lines of battle or to define a specific enemy. Thus, his critics claim, we are left in a political no-man's-land, threatened from all sides but with no one in particular to fight and with the ensuing predilection for apathy rather than action. Foucault, in fact, recognized this problem but was unwilling to alter the formulation. He stated, "My problem is not that everything is bad, but that everything is dangerous, which is not exactly the same as bad. If everything is dangerous, then we always have something to do. So my position leads not to apathy but to a hyper- and pessimistic activism. I think that the ethico-political choice we have to make every day is to determine which is the main danger" (Foucault 1983b, 231–32). Unending action is required to combat ubiquitous peril. Such pessimistic hyper-activism is precisely the stuff of a politicized tragic heroism. One acts not because goals are attainable but because it is one's fate to struggle valiantly. One struggles because the uncontested life is deemed not worth living. *Everything* is dangerous precisely because all systems of power, all forms of social and political organization inhibit struggle, they militate against their contestation. Our reflections on the nature of this activism may be guided by an understanding of the essentially agonal character of Foucault as a thinker.

With Nietzsche, Foucault maintained the agonal nature of the philosophic pursuit. Truth is not the discovery of dispassionate inquiry but the product of fierce struggle, the spoils of a victory. Its nature is always partisan. We discover that "the subject which speaks in this discourse cannot occupy the position of either jurist or philosopher—that is, the position of a universal subject. In this struggle of which he speaks he is inevitably on one side or the other. He is in the battle, he has adversaries, and he fights for a victory. And if he also speaks of truth, it is this perspective and strategic truth that permits him to claim victory" (Foucault 1976–77, 363). Foucault, as one commentator aptly noted, "hyperpoliticizes consciousness" (White 1988, 199). One is forced to recognize the partisan nature of knowledge. "Truth," Foucault claimed, "is never politically indifferent or useless" (1976, x). Yet the politics of truth, it would seem, is highly individualistic. It is the nature of modern thought, Foucault had already asserted in *The Order of Things*, that it is no longer merely theoretical and "cannot help but liberate or enslave." He goes on to say that Nietzsche, among others, understood this. But the irony is that this realization is for many an "involuntary profundity." For in their "profound stupidity" they assert that all thought must express the ideology of a class, must be either "progressive" or "reactionary." Modern thought is indeed inextricably political, but its politics are not to be dichotomized along class lines or identified by its occupation of a position within the traditional political spectrum (Foucault 1970, 328).

The political nature of knowledge means that it cannot but have political uses and tendencies. But just what these are to be or will become is not to be decided on the basis of extraneous social and political theory. Such a conceptual constriction would abet the ploys of subjectification, that is, the strict identification of social agents as unrecalcitrant bearers of determined political roles and structures. The point for Foucault is not so much to organize struggle by defining its purpose as to stimulate its potential creativity. His works, necessarily unprogrammatic, are nonetheless critical. He explained, "Critique doesn't have to be the premise of a deduction which concludes: this then is what needs to be done. It should be an instrument for those who fight, those who resist and refuse what is. Its use should be in processes of conflict and confrontation, essays in refusal. It doesn't have to lay down the law for the law. It isn't a stage in a programming. It is a challenge directed to what is" (1978b, 13). With this in mind, Foucault identified the agonal nature of his own "gay science": "It is thus necessary to bring into the struggle as much gaiety, lucidity and determination as possible. The only sad thing is not to fight. . . . Writing interests me only in the measure that it incorporates the reality of combat, as an instrument, a tactic, scouting. I would like my books to be like lancets, Molotov cocktails, or minefields, and have them burn up after use in the manner of fireworks" (1985b, 14).[10] Foucault's description of his texts as tool kits bears out this understanding. With a flourish reminiscent of Nietzsche's perspectivist challenge to his readers, Foucault outlined how his own intellectual engagement was designed as a provocation: "All my books . . . are, if you will, little tool kits. If people wish to open them and make use of this certain phrase, idea, or analysis, as one would use a screwdriver or a wrench in order to short-circuit, disqualify, or break the systems of power, possibly including even those from which my books are conceived . . . well, so much the better" (Interview in *Le Monde*, 21 February 1975; see also 1980, 145).

Foucault perceived his own writings as weapons of struggle. Most importantly, this struggle entails the ironic task of confronting the systems of power from which one's own thoughts and perceptions draw their support. Hence, the iconoclastic nature of Foucault's work. For his targets are not simply the evils of oppression but the modern sacred discourses of rights, sovereignty, and humanism in which, he claimed, present-day power has found its foothold. By giving us the illusion of a realm of freedom untouched by power, these discourses contribute to our further entanglements in its webs. When dealing with the modern period, he maintained, "the problem is to free oneself from it, . . . one has to dig out a whole mass of discourse that has accumulated under one's feet. One may uncover with gentle movements the latent configurations of earlier periods; but when it is a matter of determining the system of discourse on which we are still living, when we have to question the words that are still echoing in our ears, which become confused with those we are trying to formulate, the archaeologist, like the Nietzschean philosopher, is forced to take a hammer to it" (quoted in Sheridan 1980, 196). The problem, his critics remind us, is that the product of Foucault's hammering is a normative and political wasteland. Without the liberal and humanist goals of emancipation and self-realization, a meaningful politics becomes impossible. One is left without foundations or supports for action, the bomb already having fallen. And any efforts at reconstruction would have to overcome the incapacitating fear that one would unavoidably be acting as an abettor to the establishment of a potentially even more pernicious regime.

However, the problem as Foucault saw it is that our orientation to the question of power is misleading. Politics should not be conceived of as a game where goods, such as freedom, form the stakes between competing forces. Freedom is inherent to the agonistic relations that constitute politics. "Liberty," Foucault said, "is a *practice*"; it is "what must be exercised" (1984a, 245). Specifically, liberty is what is experienced in struggle, for only then are constraints recognized, challenged, and overcome. We are told,

> Rather than speaking of an essential freedom, it would be better to speak of an "agonism"—of a relationship which is at the same time reciprocal incitation and struggle; less of a face-to-face confrontation which paralyzes both sides than a permanent provocation. . . . For to say that there cannot be a society without power relations is not to say either that those which are established are necessary, or, in any case, that power constitutes a fatality at the heart of societies, such that it cannot be undermined. Instead I would say that the analysis, elaboration, and bringing into question of power relations and the "agonism" between power relations and the intransitivity of freedom is a permanent political task inherent in all social existence. (1983b, 222–23)

Despite Foucault's unwillingness to theorize on man's transcendental nature and his inability to support a humanist discourse and justify its political positions, there is to be found within his work a consistent political orientation. Yes, the webs of power/knowledge are inescapable. Yes, our words and deeds, moral and political, are always suspect for their tainted origins and strategic implications; for there is no philosophical realm from which we might speak truth to power. But no, we are not then left without any premise or purpose for our actions. There is indeed a legitimation for struggle, namely, the perpetuation and amelioration of the conditions that make struggle itself possible.

In other words, Foucault's political project is founded on the valorization of struggle. It is the human condition to exist within a system of power; it is the human potential to incessantly resist its reach, relocate its boundaries, and challenge its authority. The ethicopolitical choice to be made every day consists in a judgment as to what form of power most threatens (the possibility of) its continued resistance. The destabilization of its hegemony then becomes imperative. As Foucault stated, "Obviously constraints of any kind are going to be intolerable to certain segments of society. The necrophiliac finds it intolerable that graves are not accessible to him. But a system of constraint becomes truly intolerable when the individuals who are affected by it don't have the means of modifying it. . . . There is no question that a society without restrictions is inconceivable, but I can only repeat myself in saying that these restrictions have to be within the reach of those affected by them so that they at least have the possibility of altering them" (1982–83, 16–17). If the humanistic—in particular the liberal—ethicopolitical horizon is defined by the sanctity of the individual, Foucault's ethicopolitical horizon is defined by the sanctity of struggle. For Foucault the aesthetics of life solicit a politics characterized by the struggles of self-creation. And these struggles have as their precondition a certain social and political forum. The space in which these struggles might occur must be created and maintained. This is the political task.

By this account many of the rights defended in the humanist tradition, such as freedom of speech, are also defensible from Foucault's standpoint. The grounds for their defense, however, are different. The humanist defends free speech as intrinsic to human dignity, as a constitutive part of what it means to be a human being capable of fulfilling its political and moral nature. The Foucaultian defense, on the other hand, does not rest on a definition of what humanity is, what its dignity consists in, and therefore what the requirements of its safeguarding are. Rather, it is based on an understanding of humanity as involved in the struggle of becoming, of creating itself. One's dignity is not that one *is* something but that one may *become* many things. If we are to use the language of rights, then, it must be with the understanding that what needs preservation or creation are the political, social, and cultural conditions under which individuals are allowed the possibility of struggling to change these same conditions. And this must include the conditions that influence their means of understanding and identifying themselves. One must be able not only to struggle but also to struggle as other, as an individual or as a member of a group whose self-constitution is at hand. Plurality is both the precondition and dynamic force of such a politics. Its trajectory, by definition, must remain undetermined. There is, Foucault has said, no absolute liberty to pursue any form such a political art might take. But in large part this is because the nature of the art is being perpetually (re)defined by its participants.

Political struggle, then, is not strictly a negative concept. It may always be construed as a struggle *against;* but it may also be a struggle *for.* Minimally, it will be a struggle for the establishing of conditions in which self-creation is made possible, in which the assertion of individuality and otherness is viable. It is in this sense that we must understand Foucault's remark that "one must not be for consensuality, but one must be against nonconsensuality" (1984a, 379). For one must not seek consensuality by deprecating struggle, not achieve it by extirpating the agonistic tension inherent to political life. Despite this, the struggle against nonconsensuality is justified. For authoritarian and tyrannical rule is what outlaws the conditions under which a true politics is made possible.

Encomiums of struggle, of course, do not preclude their use for purposes quite antithetical to their author's intentions. The political spectrum of Foucault's critics, as well as that of the epithets he drew from them, bears witness to the practical nonspecificity of his work. Foucault, however, was little concerned with this charge; for, as he pointed out, whatever one chooses as a thematic imperative (justice, consensuality, community, reason, liberty, progress, humanity, faith, or whatever) the possibility of its cooptation, of its appropriation for vastly divergent programs, is analytically obvious and historically demonstrable. Indeed, Foucault's own work bears out this fact while confronting its critics with a *tu quoque.* Foucault's independence from Nietzsche's antipolitical and aristocratic positions is gained through Nietzschean means—perhaps a fitting repayment to such a teacher but one that would indeed have made him groan and protest. In turn, the charge that Foucault was an irrationalist, for example, would appear to be based less on any discernible doctrine of emotivism in his work than on his critics' irritation at the effect of his writings. For they prompt a recognition of the unlimited range of the insidious uses of modern reason and rationality.

The pathos of struggle that characterizes Foucault's work provides its own best defense. For the cooptation of discourse is only disconcerting if its participant looks with anxious and tired eyes to the end of the struggle of which his writings partake. Foucault neither foresees such an end nor frets for its absence:

> Won't everything that is said be inscribed in the very mechanisms that we are trying to denounce? Well, I think it is absolutely necessary that it should happen this way: if the discourse can be co-opted, it is not because it is vitiated by nature, but because it is inscribed in a process of struggle. Indeed, the adversary pushing, so to speak, on the hold you have over him in order to turn it around constitutes the best valorization of the stakes and typifies the whole strategy of struggles: as in judo, the best answer to an opponent's manoeuver never is to step back, but to re-use it to your own advantage as a base for the next phase. (1985b, 2)

Political discourse presents itself as a participant in a vastly intricate and unending match of martial arts. Victory is always tantalizing, but the activity itself proves its own reward, in large part because for Foucault politics is an *art*, an aesthetic agonism, a sort of heroic venture within an overarching aesthetics of life.

Struggle for its own sake: but does this motto of Foucault's *ars politica* not facilitate its adoption by propagandists of fascist politics interested in such a heroization of struggle? Foucault thought not. For the aim is quite the opposite of an increasingly systematized accumulation of power. Indeed, Foucault identified his prescribed politics—a joyful, polymorphous, unhierarchical, creative, and self-constituting engagement—as precisely that aspect of the "art of living" that would "ferret out" and counteract the fascism "ingrained in our behavior," in "our speech and our acts, our hearts and our pleasures" (1983a, xiii). Foucault's insistence on the propagation of this politics is in large part what distinguishes him from Nietzsche and what makes him less susceptible to an appropriation by fascistic adversaries who, in their Nazi incarnations, had claimed Foucault's anti-Teutonic mentor as their own.

In opposition to the "terrorists of theory," the "bureaucrats of the revolution," and that "fascism in all of us," Foucault refused to lay out a programmatic framework for praxis (1983a, xii–xiii). But he was explicit with regard to the nature of struggle and its inherent dangers. Generally speaking, there are, according to Foucault, three types of struggle: against forms of domination, against exploitation, and against subjectification. Presently the struggle against subjectification—"against that which ties the individual to himself and submits him to others in this way"—is predominant (1984a, 212).[11] Domination (in feudal societies) and exploitation (in the nineteenth century) were previously in the foreground and constituted an "objectification" of man; they persist, Foucault is quick to add, in an abated form today. Humanism, especially in its liberal incarnation, has been largely responsible for the mitigation of domination and exploitation. The danger of fighting under the humanist banner today, however, is that the struggle to escape forms of domination and exploitation is often carried out in thrall to the mechanisms of subjectification. The alternative is not to abrogate liberal institutions and end discussion that the struggle against subjectification might erupt. Rather, the point is to instill—by means of speech and action that is sustained by, but not restricted to, liberal institutions—an agonistic and aesthetic sensibility.

The parallel between political and intellectual struggle is clear. "Do not ask who I am, and do not ask me to remain the same," Foucault had told his readers (1972, 17). For it is only by his frequent wielding of a hammer against the foundations of his own thought that his protean identity took on its forms. Man, Foucault might have said, echoing Nietzsche, is something to be created and overcome. This never-ending feat, so evident in Foucault's own intellectual endeavors, has its counterpart in his understanding of struggle as the ultimate defense of politics. For the power we cannot escape is not omnipotent. Its relationships are "nothing other than the instant photograph of multiple struggles continuously in transformation" (Foucault 1989, 183, 188). And these struggles both prepare the ground for, and manifest, our creative freedom. Political engagement, by this account, is primarily about the preservation and enhancement of an essentially conflictual and ongoing process of self-definition and social definition. Foucaultian politics, in effect, is simply the broadening of the field of participants engaged in this process.

The struggles involved, as previously noted, are not fought along class lines. Still, it can be misleading to characterize Foucault as having individualist politics. For the individual, the subject, is held not to be a stable identity but the ductile product of struggles, of ongoing relations of power/knowledge and resistance. Who, then, Foucault was asked, opposes whom in this politics of struggle? His answer reflects Nietzsche's own speculations on the agonism and multiplicity of the soul: "This is just a hypothesis, but I would say it's all against all. . . . We all fight against each other. And there is always within each of us something that fights something else." Ultimately, Foucault went on to say, the question, Who opposes whom? must be answered with *individuals* (or even *subindividuals)* (1980, 208). The *bellum omnium contra omnes* is indeed boundless; its dynamics have as much to do with the formation of individuals as with the formation of states.

Foucault's work might be seen as the antithesis of the Hobbesian project. Hobbes hopes to escape the war of all against all by embracing a Leviathan whose powers (over language, among other things) are such that conflict is diminished if not made quite impossible. Foucault set out to demonstrate our participation in the life of a headless Leviathat that has perfected the techniques of its Hobbesian forebear. He prompts a recognition of the benefits of agonistic interaction within the modern Leviathan so that we might loosen its grip. The goal, of course, is not civil war but a stimulation of politics such that our struggles (rather than their imposed absence) become our defining characteristic.

If Foucault's project is the opposite of Hobbes's, it is also a twist on the liberal effort that grew in the wake of Hobbes's stark alternatives. Hobbes sacrificed freedom that there might no longer be struggle. Liberalism accepted the need for struggle that there might be freedom. Foucault attempted to show that freedom and struggle necessarily coalesce. Here, then, is the crux of the matter between Foucaultian politics and liberalism: the latter most values freedom and posits political struggle as its necessary prerequisite; whereas Foucault most values struggle and posits political freedom as its concomitant, albeit elusive, achievement. But this is not to say that any and all forms of struggle are equally meritorious or desirable. The enhancement of struggle—not its mere production or exacerbation—must form the criterion of political judgment. The struggle for inordinate wealth, for instance, might justifiably be curtailed; for it signifies in its fruition the disempowerment of the impoverished members of the political community. The struggles of the Ku Klux Klan are also condemnable; for, even apart from their patent ugliness, they are aimed at the exclusion of certain citizens from the forum of politics. Foucault's position does not lead to an abandonment of all means of evaluating struggle. Rather, it extracts these evaluations from an understanding of benefits of political agonism. The point, moreover, is not to valorize struggle by creating or condoning the conditions of its most egregious demand. The Gulag may indeed present its inhabitants with the most pressing need for struggle, but the Gulag certainly does not enable the proliferation of struggle.

There is, then, an irony in the critic's lament that Foucault has left us with no reason to fight. For it is precisely the enhancement (and one might add, sublimation) of struggle that is Foucault's chief concern. Hence the *will to struggle*, not the *reason for struggle*, is the focus of Foucault's work. It is the inextinguishable pathos of struggle within man that must be pitted against the vast matrices of disciplinary power which threaten to overwhelm and homogenize. Admittedly, Foucault's eulogy of struggle often appears to restrict itself to the "heroism of knowledge" lauded by Nietzsche. And Nietzsche's own antipathy to political engagement—indeed, his contempt for it—may be found lingering in Foucault's work. But it lingers as a suspicion directed at politics conceived strictly as a goal-oriented activity. For Foucault politics is redeemed because it is less about the achievement of goals than the struggle to transform the conditions of their conception and articulation. Politics is the confronting of the conditions under which we come to define ourselves by these same goals. And it is in this confrontation, he asserted, that we come to know freedom. There are no final resolutions to the quandaries of political life. But there is much to be learned in how best to live such an unresolved existence.

The issue here is not "positive" versus "negative" liberty, that is, correct consciousness and self-mastery versus a straightforward freedom from constraint. Foucault eschewed the concept of positive liberty as naive and delusory, for knowledge and subjectivity are always already the products of power. In turn, he found the securing of negative liberty to be insufficient, for it fails to address our relationship to modern forms of power completely, forms which, by and large, have implanted themselves behind the aegis of our rights. Foucault identified liberty neither solely as the absence of the hindrances to action nor as the fulfillment of the psychological preconditions for rational action. It is found in action itself. In a manner reminiscent of Aristotle's understanding of the pleasure of virtuous activity, Foucault identified liberty as what arises from struggle, ceases with prolonged inactivity, and is not so much the goal of struggle as its supervention.[12]

Freedom, Foucault observed, is most manifest in those courageous, sometimes self-sacrificing, actions wherein the stake in the struggle is life itself. And this heroism, according to Foucault, is not restricted to great men. Foucault asserted, "No one has the right to say, 'Revolt for me, there's going to be an ultimate liberation of all men.' But I do not agree with those who would say: 'It is pointless to have an uprising, things will always be the same.' You don't lay down the law to someone who risks his life before a power. Does or doesn't one have reason to revolt? Let us leave the question open. The fact is there are uprisings, and it is through this that subjectivity (not that of great men but that of whomever) introduces itself in history and gives it its breath" ("Inutile de se soulever?" *Le Monde*, 11 May 1979).[13] Perhaps more important than the heroic manifestation of freedom, however, are the more or less continuous acts of resistance and political struggle that serve to loosen the hold of power enough that glimpses of freedom are made possible. Foucault's concern was to understand and promote this spirit of struggle, a spirit whose incarnations of resistance perhaps have never been more crucial than they are today in a world where the shell shock of life has never been more debilitating.

In Nietzsche the absence of God prompted a revitalization of the heroic. With Foucault, the deconstruction of the subject prompted the commendation of the act of resistance and the call for a politics of struggle. I have attempted to illustrate how Foucault's agonism may better be understood with reference to Nietzsche's tragic heroism and subsequently how Foucault's concern with the creation of the self as a work of art was prompted by the same genealogical deconstruction that led Nietzsche to insist on living life aesthetically. My object has not been to describe Foucault as Nietzsche's epigone. Foucault's writings exhibit more of a selective adaptation than an uncritical adoption of Nietzsche. Nietzsche's struggle to order his soul led him to disdain and depreciate politics. Foucault opted for its proliferation. In short, Foucault politicized what Nietzsche had internalized: the will to struggle. Conceived not as the desire to dominate, exploit, or subjectify, this will to struggle is rather an eagerness to confront and grapple with individuals as other. Indeed, it is a celebration of their otherness as that which, along with one's own multipartite nature, allows history its breath, politics its life, and spirit its nourishment.

Notes

1. Thus, Foucault is categorized as a voice of postmodernism; for, as Lyotard succinctly put it, the postmodern may be defined as "incredulity toward metanarratives" (Lyotard 1984, xxiv).

2. This is not to say that "intellectual" acts of resistance are the only ones available to us. Foucault was very much involved in practical politics and consistently condemned defeatist attitudes within this realm. A particularly good sampling of Foucault's thoughts on particular issues can be found in Foucault 1988. For an informative analysis of Foucault's political engagement and an attempt to link it to his theoretical position see Gandal 1986.

3. Likewise, Nancy Fraser is most disturbed by Foucault's apparent refusal to answer this same question. A similar anxiety is what underlies the critiques of most political theorists: "Foucault calls in no uncertain terms for resistance to domination. But why? Why is struggle preferable to submission? Why ought domination to be resisted? Only with the introduction of normative notions of some kind could Foucault begin to answer such questions" (Fraser 1981, 283).

4. Describing Foucault's relation to Nietzsche presents its own problems. Although Nietzsche spent much of his time telling us who he was, his purpose was to display a man of many masks. Foucault also bears an unstable identity. He has been labeled everything from an anarchist to a neofascist. Moreover, he has delighted in the anonymity allowed by the protean nature of his writings. Mine is not one more attempt to categorize Foucault's politics by situating him within the traditional political spectrum. In fact, part of my purpose is to argue that any such categorization necessarily will miss the mark.

5. Danto's claim that "Nietzsche was an inveterate worshipper of heroes," (Danto 1965, 198) reflects a very common misunderstanding.

6. The section here concluded is an abbreviated account of the first chapter of my book, *Friedrich Nietzsche and the Politics of the Soul* (Princeton: Princeton University Press, 1990), and appears here by arrangement.

7. Foucault was convinced of the perniciousness of any standardized moral alternative to an aesthetic perspectivism. He said, "The search for a form of morality acceptable to everybody in the sense that everyone should submit to it, strikes me as catastrophic" (1985a, 12).

8. Presupposed in such a conscious deformation is a thorough understanding of the subject matter, lest the "use" of Nietzsche becomes a travesty rather than a tribute.

9. While consistently employing this terminology, Foucault maintained that this characterization of politics was hypothetical. Indeed, by the 1980s he had come to speak of politics less as war and more as government, as a "pastoral power," thus undercutting the perception that politics was a matter of the direct confrontation of polarized forces. See my article, Thiele 1986.

10. Emended and translated from the original in *Nouvelles Littéraires*, 17 March 1975, p. 3.

11. Thus, genealogy was defined as an accounting of the constitution of subjects and the accompanying struggles (Foucault 1980, 117).

12. Participatory democrats have, of course, always insisted that political activity is properly never merely a means but, at least in part, an end in itself. Indeed, their theoretical genealogy evidences Aristotle as a reluctant grandfather. The importance of Foucault, I propose, is not that he discovered political struggle to be its own regard but that he came to terms with the ultimate stakes in the game: not solely the preservation of our dignity and the practice of our liberty, but our ongoing self-constitution and social constitution.

13. The idea that freedom is ultimately manifest in the willingness to risk one's life appears as well in Foucault's *Tanner Lectures.* Foucault stipulates that a man's conduct is never exhaustively determined by power relations, for he always has the option of nonsubmission and death: "If he can be induced to speak, when his ultimate recourse would have been to hold his tongue, preferring death, then he has been caused to behave in a certain way. His freedom has been subject to power. . . . There is no power without potential refusal or revolt" (1981, 253).

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CONCRETE THEORY: AN EMERGING POLITICAL METHOD

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Concrete theory is defined by a cluster of attributes—emphasis on governmental and other political elites, on strategic decision-making processes freed from narrow notions of economic rationality, and on a concern with the environment and institutions within which choice occurs. The approach has been observed recently in all research-oriented subfields within political science. Eight exemplars are discussed. Concrete theory demonstrates a novel combination of strong interest in empirical political processes, formalized through models that emphasize logical structure and depth explanation. Its effect is to bridge the gap between behavioral and institutional approaches.

> The task of science . . . is to demystify experience and simplify it, not to extol its complexities
>
> —Gerald H. Kramer

In the era of eclecticism that has followed behavioralism and postbehavioralism the political science discipline has directed far less attention to what was once a favorite pastime, self-analysis. Although there are exceptions (Ball 1987, Ricci 1984, Seidelman and Harpham 1985), the discipline has recently preferred to "get on" with its work (Finifter 1983, Weisberg 1986) rather than struggle over its premises.[1] This trend is certainly a healthy one, yet inattention also runs the risk that useful new ideas may be overlooked.

One recent innovation in political science is a tendency to which I give the name *concrete theory.* While it may seem surprising to describe theory as concrete when it is usually considered inherently abstract, the designation is an apt one. It denotes a kind of theory that rather than rising above the political process to form conceptual *frameworks* by means of which events can be categorized (Boynton 1980; Coleman 1964; Easton 1965; Kemeny and Snell 1978; Parsons 1951; Simon 1957; Wallerstein 1974–80), instead cuts to fundamental behavioral *processes* that underlie political outcomes (Harre 1970, 26–28; Hempel 1965; Lave and March 1975; Stinchcombe 1968).

The two types of theory—the abstract and the concrete—may be equally scientific, but they develop their concepts in quite different ways. The abstract approach mounts from reality to ever higher (and emptier) concepts. The concrete approach equally achieves universality; but by burrowing, not rising. It strips away all inessential attributes and lays bare a central process that underlies and serves to explain observed political events.[2] Our present inquiry suggests that concrete theory is an original development in political science, especially in its bridging of the behavioral and institutional traditions.

The concrete approach is no stranger to political science. Early examples of its spirit can be recognized in such well-known works in political science as Barnard's organizational model, (1938, 46–61), Bentley's political "process" models (1908), Neustadt's 1960 presidential model (2d ed. 1980, 131), Key's theory of party genesis (1964, 200–201), Fenno's study of Congressional committee dynamics (1966, 681–88), Huntington's analysis of national development and decay (1968, 72–74), and Apter's theory of modernization (1972, 415–24). These works share a vigorous grasp of the living political process, unhampered by analytical preconceptions.

A causal next step in the formation of concrete theory, giving a new strength to the direction initiated by these early works, was taken by the classical generation of rational choice theorists (Buchanan and Tullock 1962; Downs 1957; Olson 1971 [orig. pub. 1965]); Riker 1962; Schelling 1960; Simon 1945). But it is important to understand that rational choice theory and concrete theory are not the same. Concrete theory borrows the logical rigor introduced by the rational choice theorists, but concrete theory's methods and assumptions are both more flexible and more deeply empirical.

Concrete theory is defined by several characteristics shared by its exemplars that distinguish it from other scientific approaches to the study of politics. While one or two of the elements may be found individually in other works, it is the presence of the *complete cluster* that distinguishes, and serves to define, concrete theory.

The major defining marks of concrete theory include:

1. a focus on actual decision makers, usually political elites, as the object of study, in direct contrast both to the survey research emphasis on mass behavior (Campbell et al. 1964, 1966) and the institutional approach (Evans, Rueschemeyer, and Skocpol 1985; Tilly 1975).

2. an expanded self-interest axiom that deepens the level of explanation by providing for a variety of political, as distinct from purely economic, goals—the decision makers are neither unreasonably rational nor cognitively out of their depth (e.g., Kahneman, Slovic, and Tversky 1982; Simon 1982; Wason and Johnson-Laird 1972).

3. close attention to the environment, especially the political institutions and the political environment within which decision occurs (Farr 1987; Popper 1972, 178–80).

4. a strong logical quality, which vigorously shapes its materials into dynamic models that capture the "action" of politics—giving concrete theory its radically explanatory quality (Lave and March 1975; Merton 1957).

5. an overriding concern with specificity (the cases I discuss deal not with institutions in general but with specific institutions, not behavior in general but specific types of behavior).

Exemplars of the new brand of concrete theorists may be found in all the subdisciplines within political science (e.g., Tullock on organizational behavior 1965, Sylvan and Chan on foreign policy decision making 1984, Stewart, Hermann, and Hermann on Soviet behavior 1989, Dogan and Pelassy on clientelism 1984, [chap. 10], Liebenow on civilian-military relations 1986, Jacobs on the dynamics of city growth, 1970).

I focus on eight examples of concrete theorizing, (taken from all political research subfields), illustrating how the approach can be applied successfully to a broad range of political phenomena. The examples include Bates on African economic leadership (1981), Jacobson and Kernell on congressional voting (1983), Miller and Jennings on presidential party conventions (1986), North and Thomas on political-economic development (1973), Przeworski and Sprague on electoral socialism (1986), Skowronek on presidential decision making (1984), Pressman and Wildavsky on public program failure (1979), and George and Smoke on international deterrence theory (1974).[3]

Mainstream empirical theory and concrete theory are complementary. Mainstream theory is stronger in some areas of inquiry and research, such as rigorous operationalization and testing; concrete theory is stronger in other areas, such as the scope, depth, and clarity of explanation. By recognizing the differences between the approaches and using each orientation when it is most appropriate, political inquiry can increase the range and depth of its grasp of the political process.

Nor does it seem premature to discuss a methodology that in fact has not been consciously defined by its practitioners. Concrete theory is too easily lost. Several of the concrete theorists included here have written other works not displaying the attributes of concrete theory (e.g., Bates 1983; Jennings and Niemi 1981; Kernell 1986; but contrast Jacobson 1980 and perhaps Przeworski 1985). This highlights the need to make an explicit definition.

Science and Theory

The appropriate canvas against which to evaluate new developments in empirical theory is not the philosophy of science but the methodology of the social sciences. The philosophers of science have directed attention principally to theory *testing* as the criterion by which science is separated from nonscience (Kuhn 1962; Lakatos 1970; Popper 1959). While this course was justified historically by the changed direction taken in political science (Dahl 1963; Easton 1953; Eulau 1963; Macridis 1955), and retains its importance (Almond et al. 1988, 872), the single-minded focus on theory testing alone is no longer a complete solution to the development of political science. As Ball argues, the attention to theory rejection can become obsessive. Political scientists, rather than giving research programmes a fighting chance, "have made them into sitting ducks; and, in a discipline which includes many accomplished duck hunters, this has often proved fatal" (1987, 34).

In trying actually to build empirical theories, political scientists have had little practical assistance; what literature exists is found largely in the neighboring social sciences rather than political science itself. I briefly review some ideas from a very short list of books about the practice of empirical theorizing (Glaser and Strauss 1967; Lave and March 1975; Merton 1957; Stinchcombe 1968) in order to provide a methodological framework within which concrete theory can be viewed.

The most familiar point at which to start is Robert Merton's often cited yet rarely studied "theories of the middle range" (1957, 5–10). Merton was a strong supporter of the idea of theory-guided research, yet "grand theory" was anathema to him. The epithet he uses is *grandiose.* This appears to be a position with which many political scientists, chastened by events of the past decades, might concur. Is there an alternative to grand theory?

Merton defined his alternative choice, middle-range theory, as "intermediate to the minor working hypotheses evolved in abundance during day-to-day routines of research, and the all-inclusive speculations comprising a master conceptual scheme" (1957, 5–6). Middle-range theories develop stepwise, as "special theories adequate to limited ranges of social data" are consolidated into "groups of special theories" in which individual theories' inconsistencies have been ironed out, the premises clarified, and the logical connections made explicit (pp. 10, 14–15). While no parallel can be wholly exact, Merton's description of middle-range theory comes very close to concrete theory. The subjects over which the explanation ranges are many, and the theory therefore has a wide scope. But it is not universal in the sense we have come to associate with grand theory, of attempting to build frameworks within which every possible event can be contained (compare Verba 1985, 34, on "islands of theory" as preferable to too-general theory).

While avoiding grand theory, however, it is important not to lose the theoretical quality itself. The attempt to avoid all abstract concepts, as found in Glaser and Strauss's "grounded theory" (1967, 259-62), may throw out the baby with the bathwater. But the idea of grounded theory is useful to demarcate the field in which concrete theory exists. If concrete theory as I delineate it were made *more* concrete, it would only degenerate into isolated concepts lacking the logical interconnectedness characteristic of Merton's brand of theory.

The newly evolving idea of "situational analysis" is an alternative to the covering law model of social science procedure (Donagan 1964; Nelson 1975). While derived from Popper (1972, 179-82), the situational model has been most recently defined within a political context by Farr as a "detailed description of (1) the natural environment; (2) the social environment (that is, other strategic actors and the social relations between actors); and (3) the problem-situation in which an actor finds himself or herself, defines his or her problems, and tries out tentative solutions" (1987, 50). That such a description is congruent with concrete theory will become clear from the examples. The parallel adds fuel to my present argument that the concrete theories identified here are not merely eccentric appearances within political science but in fact represent an important new methodological direction (see Watkins 1970; Wisdom 1970).[4]

Concrete Theories

Concrete theories are not difficult to summarize. Indeed, one of their most engaging qualities is their simplicity. Where other forms of theory require a considerable commitment of time and memory before one can appreciate their often subtle meaning, concrete theory is short and neat. Therein it fulfills a common methodological requirement for theory—one rarely met by other types of political theory—its simplicity should help researchers order and explain their data better (Dogan and Pelassy 1984; Kramer 1986).

Jacobson and Kernell

In *Strategy and Choice in Congressional Elections* (1983) Jacobson and Kernell address one of political science's central concerns, the question why voters vote as they do. The larger issue in this field has been the rationality—or at least the common sense—of the U.S. voter (initially, Campbell et al. 1964; Key 1966). The immediate question posed by Jacobson and Kernell is narrowly defined and involves a paradox in empirical studies of voting behavior between aggregate-level data indicating that voters vote according to economic (or political) conditions and individual level survey data showing voters making decisions on a variety of personalistic and local grounds.

Jacobson and Kernell turn to politically active elites, their strategic behavior, and their roles as connecting links between national conditions and individual voter choice to resolve questions raised by the apparent conflicts in the data. Their "strategic politicians" theory argues that office holders, challengers, party leaders, and financial backers all make personal utility calculations about upcoming elections based on national economic and political conditions early in the election year. Good times draw out highly qualified candidates, as well as large financial contributions and strong organizational efforts; in bad times, everyone keeps a low profile and makes only token campaign gestures (pp. 20-23). In sum, the voters believe (indeed, correctly) that they are merely "voting for the best person," while in fact national economic and political conditions have determined the outcome.

On the question of parties and their role in the U.S. political system, Jacobson and Kernell draw from their analysis an unorthodox conclusion. They do not join those who complain of the loose U.S. party system or those who regret its recent "decline." Jacobson and Kernell suggest that the failure to meet the strong-two-party parliamentary ideal is not necessarily a weakness but may be economically rational (p. 86).

African Agricultural Problems

Bates points out in the introduction to his *Markets and States in Tropical Africa* (1981) that economists steadfastly retain the faith that given the appropriate incentives, peasants in underdeveloped countries will "turn sand into gold." This faith has been severely strained by the results of postcolonial developments. Whereas development theorists (Almond and Coleman 1960; Apter 1965; Chilcote 1981, 272–87; Janos 1986) believed that given the knowledge available to guide the new nations, modernization was a process characterized by a certain inevitability, the actual outcome has been political disarray and economic regression. (See the reflections by World Bank advisors in Meier 1987 and Meier and Seers 1984; on African leadership see, most recently, Bienen and Walle 1989).

Bate's solution to the question how this result has occurred so universally is sufficiently compact that it deserves to be quoted in his own words:

> Fledgling industries locate in the urban areas. Workers and owners, while struggling with each other for their share of industrial profits, possess a common interest in perpetuating policies that increase these profits. They therefore demand policies that shelter and protect these industries. They also demand policies that promise low-cost food.
>
> Because of the public purposes they espouse, African states seek to advance the interests of industry. To secure revenues to promote industry, they therefore seek taxes from agriculture. By maintaining a sheltered industrial order, they generate economic benefits for elites, as well as resources for winning the political backing of influential groups in the urban centers. . . .
> . . . For the benefit of others, [the farmers] are subjected to policies that violate their interests. But the effects of these policies are increasingly harmful to everyone. Reducing the incentives to grow food leads to reduced food production; the result is higher food prices and waves of discontent in the urban centers. (Bates 1981, 120, 129).

As Bates notes, the logic is "simple but powerful."

Przeworski and Sprague

Students of socialism have sometimes despaired of finding any scientific hard ground in a subject area covered by the marshes of ideology. The third example of concrete theory, Przeworski and Sprague's recent *Paper Stones* 1986, stands as an exception. (The title reflects the early socialist belief that ballots would supersede violence as a path to socialism.) *Paper Stones* centers on one of the most vehemently debated questions in the socialist literature, the possibility of a "parliamentary road to socialism," that is, a peaceful revolution.

There have been two approaches to this topic: (1) the Marxist polemical literature, which is both deductive and normative (Gramsci 1971; Lukacs 1971); and (2) the field of comparative voting studies (Lipset 1981; Rose 1980). In contrast, for Przeworski and Sprague the formative element shaping objective conditions into politically relevant conditions is the political parties. These party elites shape the nature of the political argument: the salience of any socioeconomic status factor, such as class, is a "cumulative consequence of the strategies pursued by political parties" (p. 9).

Despite the potential generality of this theory as an explanation of total political systems, *Paper Stones* employs it only as it relates to the central topic, electoral socialism. For parties of the Left the attempt to shape the objective factors into workable political form is irresolvable, since the existence of Socialist parties ameliorates the condition of the workers and the better conditions become, the less urgent is the need for socialism (pp. 1–11).

North and Thomas

The Rise of the Western World by North and Thomas (1973) is oddly slim when the topic would seem to call for greater tonnage. This results partially from its being, according to the authors, an "agenda" rather than an exhaustive study. But the method employed is also a reason: concrete theorists typically strip subjects to essentials.

The work begins with a question that has often been slighted, Why do some societies "fail"? By the seventeenth century Holland and England were flourishing, France was second-rate, and Spain and Italy were "losers" (pp. 103–15). Economics provides a preliminary answer, for "efficient economic organization is the key to growth." *Efficient* means that property rights are so organized that the opportunity for private (personal) gain leads people to undertake activities that entail social gains as well. The decisive political question, then, is, Under what circumstances do governments organize property rights efficiently? (pp. 1–3).

Historically, property rights are inextricably linked to kings and taxes. The dynamics of the North and Thomas model arise from the specific circumstances and problems faced by the sovereigns in the various European states: (1) the structure of the economy (e.g., international trade is easier to tax than domestic trade); (2) the vigor of the political competition (e.g., England did not face challenges such as the Dukes of Burgandy posed to the French throne); (3) the constituents' gains from central government (greater in centralized France than in decentralized England, where order was maintained at the local level). Within these constraints, kings and parliaments bargained for control. Monarchs who needed too much money too fast (oversized government, personal luxury, or war) found it tempting to leave (inefficient) feudal taxes in place; and modern economic relationships failed to come to the fore. Where government was highly administered and therefore expensive, heavy taxes stifled development.

Presidential Decision Making

Stephen Skowronek's "Presidential Leadership in Political Time" (1984) is somewhat different from other examples of concrete theory presented here in that it does not illuminate an interaction among several elite decision makers but rather deals with single actors, namely, U.S. presidents. The interactive focus is implicit, however: for Skowronek the president plays a kind of "game against nature" (defined as the era's whole political-institutional structure). This departs from the "textbook president" approach (outlined in Cronin 1980) but is not without forebears (Barber 1977; Neustadt 1980 [orig. pub. 1960]).

The Skowronek thesis is that a political system is characterized by institutional cycles, involving stages of construction, maintenance, and decline; and that individual presidents can only be understood in the light of the strategies they espouse in response to the situation they specifically face. While there are other cyclical or time-anchored theories (Barber 1980; Hargrove and Nelson 1984), Skowronek differs in the focus on elites rather than on, say, psychological cycles of innovation and fatigue in the electorate.

Political periods "are marked by the rise to power of new political coalitions, with one, in particular, exerting a dominant influence over the federal government." But in the course of history, each coalition decays internally because of "conflicts among interests within the dominant coalition"; and national problems and needs change as well. Increasingly, the regime depends for support on "sectarian interests with myopic demands." The longer it survives, the more it is enervated and unable to cope with current problems (pp. 87–88).

Presidents (or regime leaders) therefore face three typical situations, which recur cyclically over time: (1) the challenge of regime construction (Jackson and FDR); (2) the problem of regime maintenance and control of conflict (Polk and Kennedy); and (3) establishing leadership in an enervated regime (Pierce and Carter) (p. 89). Skowronek evaluates each of the three pairs of presidents according to this theory, and is able to show how the outcome of each administration was structured by its institutional circumstances.

Miller and Jennings

Parties in Transition by Miller and Jennings (1986) is rich in empirical results. Indeed, the very scope of the empirical analysis is apt to obscure the authors' approach, which is importantly and fundamentally different from earlier studies of national party elites. Their interest is less in the actors in isolation, and more on the institutions within which the actors are enmeshed. The research begins as a replication of earlier studies of national party presidential convention delegates (Kirkpatrick 1976). This level of analysis is itself innovative, in moving away from the party-in-the-electorate approach and the pervasive influence of theories of party realignment (see the survey in Petrocik 1981).

While the importance of the candidate is a truism with which few would quarrel (Asher 1988; Kessel 1980), Miller and Jennings' innovation is to define the internal processes of political parties as based on the highly personal interaction of specific political people. Rather than presenting national conventions as part of a grand, orderly process or as a more or less abstract scramble for political profit, many of Miller and Jennings' findings relate more "to the somewhat idiosyncratic fates of particular candidacies" than to the structural aspects of the two-party system (pp. 19–20).

It is especially interesting that the authors persuasively document the existence of strong ideological differences *within* the parties: "The coalitions we observed [within each party] were not simply the result of certain candidates exuding great personal charm, or of party or interest group leaders playing the role of power brokers. Rather the groupings stand for *basic divisions of thought* about matters of public policy and the political process. Like-minded campaign activists, mainly through a process of self-selection, gather around attractive candidates who ostensibly share their ideological views" (p. 247, emphasis added).

Policy Implementation

Pressman and Wildavsky were not the first to notice that public programs fail, but their 1979 study is unusual because it takes failure fully seriously. The Oakland Project, initiated in 1965 by the Economic Development Administration (EDA) and designed to improve economic opportunities in the city of Oakland, began (as the authors describe in detail) with widespread national and community support. It ended in collapse. One sector of the program elicited the unhappy headline, "US Invests $1,085,000 To Create 43 Oakland Jobs" (Pressman and Wildavsky 1979, 82). While this was not the whole picture, neither was it altogether misleading.

The model developed in the course of the Oakland analysis centers on (1) the elite groups involved in the Oakland program, (2) their widely diverse goals, (3) the unexpected obstacles caused by the interactive process between actors and their goals, and (4) the logic of program breakdown. The authors are at pains to emphasize that the Oakland Project was not a controversial one: there was no local opposition to EDA's plans, and the program was "everyday" and "prosaic" in character (p. 93). This serves to highlight the eventual failure.

While there was no overt controversy, however, the participants, including several federal agencies, local politicians, and local businesspeople, were subtly different: some desired a showcase success in reducing unemployment, others sought business opportunities, others were concerned with fiscal regularity and efficiency, and so on (pp. 95–97). Over the course of time ends and means changed and became entangled, which Pressman and Wildavsky consider not pernicious but natural: "Allow enough time to elapse in a rapidly changing external world and it is hard to imagine any set of agreements remaining firm" (p. 92). And this occurred despite EPA's vigorous efforts to achieve unity and coordination on the noncontroversial project. Programs fail even when there is no apparent reason:

> Looking back at the very array of governmental programs that are in operation, we conclude that they must have been implemented. Why, then, should our new program fail in implementation when so many others that fill the landscape have evidently succeeded? It is easy to forget (perhaps because we never knew) about their initial difficulties. The years of trial and error that led to the present state of operation are lost from view. . . . Programs that started out to accomplish one set of objectives end up accomplishing another for which, long after the fact, we give undue credit for implementation. (pp. 113, 116)

Deterrence Theory

The concluding example of concrete theory comes from the field of international politics and is distinctive in its explicit recognition that its methodology fits none of the usual categories. George and Smoke (1974) describe their work in deterrence theory as both historical and inductive and contrast it with two familiar, alternative methodologies: "abstract deductivist" theory, which they find misleading, and "statistical-correlational" analysis, which they find disappointing (pp. 88, 503, 513). They also distinguish their approach from case studies (pp. 92–94). In analyzing a wide array of foreign policy events including the 1948 Berlin blockade, the outbreak of the Korean war and Chinese intervention, the Hungarian Revolution, the Eisenhower Doctrine in the Middle East, and the 1962 Cuban Missile Crisis, the authors criticize Cold War deterrence theory not only for its excessive simplification but also for its excessive emphasis on threat and punishment.

As an alternative to such narrower approaches they propose to revise deterrence theory into a more widely applicable "influence theory," covering multilateral strategic interactions of both a positive and negative nature: "What is needed is not merely a better deterrence theory per se but rather a broader theory which encompasses deterrence as one of a number of means that can be employed, separately or in some combination, to influence conflict processes and to control the conflict potential in inter-state relations" (p. 591).

Their new deterrence model involves two conceptual actors, the *defender*, who is trying to maintain the status quo, and the *initiator*, who is trying to change that status. The interaction of these two actors within the crisis situation is defined by three conceptual stages—commitment, initiation, and response—each of which is linked to the others in a dynamic algorithmic, or "iterative," flow (pp. 98–102, 550). Traditional deterrence theory, the authors note, works primarily with only the first of these stages (p. 551).

When the three phases are seen altogether, the process of deterrence can be described in a flow chart (pp. 98–102) in which the two actors cyclically estimate each other's intentions and actions, engage in interaction, and revise their estimates and possibly their goals and commitments as the action develops (p. 579). This, of course, brings the deterrence model near to the boundary of a general political interaction model.

Evaluation

Is concrete theory truly original? Is concrete theory viable?

While, as a general rule, it is difficult to define a scientific mainstream unless real revolution is in the air (Easton 1965; Macridis 1955)—not the case here—March and Olsen (1984) come very near to achieving such a description. It will serve as a yardstick in attempting to locate and to evaluate concrete theory. This masterly summary and critique is especially useful because it provides not only an analytic overview of political science since the 1950s but also its major contemporary challenger, the "new institutionalism." By defining these two frameworks with precision March and Olsen's analysis lets us compare concrete theory against both its alternatives and thereby answer the question of concrete theory's originality. The comparison will show that concrete theory is inconsistent with both behavioralism and with the new institutionalism but has footholds in both camps and serves to link them.

Concrete theory's originality appears to result from two characteristics. First concrete theory is individualistic, as is behavioralism; but concrete analyses are directed to a different category of politically active individuals—elites rather than citizens. Second, concrete theory is, in every case described above, wholly embedded in an institutional perspective. This is not to say concrete theory straddles the disciplinary fence, borrowing a bit here and an opposite bit there, condemning itself to a verdict of opportunism or eclecticism. Rather, concrete theory tends to show that the polarities are not as distinct as they have seemed. A point-by-point comparison illustrates this conclusion.

March and Olsen contrast behavioralism and institutionalism on five dimensions (1984, 735–40):

1. *Contextualism.* Behavioralism considers the polity integral to society, with little independence of this environmental context; institutionalism defines the state as independent, acting on society as an autonomous force.
2. *Reductionism.* Behavioralism assumes that collective behavior can be reduced to, and explained by, individual behavior; institutionalism sees institutions acting through individuals to ensure satisfaction of institutional needs.
3. *Utilitarianism.* Behavioralism defines rational actors as calculating to maximize utility in uncertain but flexible environments; institutionalism emphasizes the importance of constraints imposed on individuals by rules, expectations, and traditions.
4. *Functionalism.* Behavioralism deemphasizes historical factors and works for "efficient" equilibriums; institutionalism has greater faith in the recalcitrance of historical events and in the constraints imposed by history in reaching solutions.
5. *Instrumentalism.* Behavioralism sees symbols as irrelevant or exploitative; institutionalism understands symbols as educational and creative of purpose and political identity.

In regard to *contextualism,* concrete theories include both the autonomy of state officials (who actively structure the political arena) and the close links between those officials and society at large (which is the source of constraints and opportunities). (See esp. Jacobson and Kernell 1983; Pressman and Wildavsky 1979; Przeworski and Sprague 1986; and Skowronek). The criterion of *reductionism* similarly shows that concrete theory partakes of both the behavioralist and the institutionalist perspectives. Concrete theorists both reduce and do not reduce: they explain aggregate behavior as the result of individual decisions, but these individuals are not abstractions such as "economic man" or even "everyman." Rather, they are different sorts of persons, each grouping of which is defined by its role in some political or institutional process (see Bates 1981; George and Smoke 1974; Miller and Jennings 1984; and Przeworski and Sprague 1986). *Utilitarianism* again puts concrete theorists on both sides: concrete theory's decision makers are rational strategic calculators. However, they calculate not in isolation but as the players of roles defined by institutions, norms, expectations, and history (see George and Smoke 1974; Miller and Jennings 1986; Pressman and Wildavsky 1979; Przeworksi and Sprague 1986; and Skowronek 1984). The conclusion is the same in reference to *functionalism*. Concrete theory sometimes finds equilibria (of which some are negative equilibria); but in other cases history cannot be escaped, and breakdown occurs before goals are reached. (Contrast Jacobson and Kernell 1983, where the rise and fall of the economy in conjunction with the goals of party elites may create stable alternating patterns, with Bates 1981, where the logic of urban-rural dynamics leads steadily downhill.) Finally, the role of *symbols* again links concrete theory to both sides of the disciplinary divide: concrete theorists "discover" that elites manipulate symbols; but this is done for a complicated range of purposes, at least some of which entail good public policy. And while they would not seem to deny that symbols are elite-serving, the concrete theorists affirm the importance of such symbols in defining the long-term life of the polity (e.g., Miller and Jennings 1986; Przeworski and Sprague 1986; and Skowronek 1984).

After reviewing these comparisons between concrete theory and the behavioral and institutional traditions defined by March and Olsen (1984), one may safely assert that fitting into neither of the major traditions of political science, concrete theory must be accorded a prima facie claim to originality; and partaking of both traditions, it cannot be said to be lacking in relevance. If concrete theory is indeed new, it is not unfamiliar to political scientists.

There remains the question of concrete theory's viability. Perhaps the most severe disability from which concrete theory suffers is that it may not appear to be "theory" at all: because it is deeply set in realistic problems and contexts, it may appear too close to "journalistic" description. Even its practitioners may seek greater formality and technical sophistication, thereby losing the dynamic, explanatory quality characteristic of middle-range theory.

Data acquisition may also become a problem for concrete theory, since data on the behavior of elites are not readily available. Elites are often difficult to approach, resistant to candid revelation, and very likely innocent about their own political motives (see Fenno 1978, app.). It is possible that the new interest in "rationalist" science on the Cartesian model (Sylvan and Glassner 1985) or the development of a new realist school (Harre 1986) will provide alternatives to empiricism. The emphasis on logical modeling among concrete theorists is indicative of possibilities in this direction. But this would require a level of commitment on the part of political scientists that cannot be predicted (see Achen 1983, 70–71, in a parallel context). The examples of concrete theory show that it is by no means fully developed as a methodology, and each individual concrete analysis raises as many questions as it answers.[5]

Concrete theory is not the vanguard for a new political science or an ultimate political methodology. But concrete theories suggest that we can make bolder use of existing materials and enrich our ability to understand both the broader perspectives of politics and the immediate, perplexing practical problems faced by nation and world (Pye 1968). Historically political scientists have tended to avert their eyes from exactly those political questions that one would have thought would be their highest concern (Ricci 1984). Concrete theorists reverse this direction. They grasp the political at its very core.

Notes

An earlier version of this paper was presented at the 1988 convention of the American Political Science Association, Washington.

1. Dryzek and Leonard (1988) contend that there is currently a plethora of self-analysis within political science, but their listing includes a number of works rather distant from the present discussion of research trends in the United States. Collini, Winch, and Burrow (1983), for instance, deal with nineteenth-century British "thinking about politics"; and Farr (1988) deals with Adam Smith and the Scots. In the same line of disciplinary history but more recent is Gunnell's (1988) illuminating discussion of the 1940–50 period in U.S. political science; but it, too, takes a metaperspective on the discipline rather than my very practical one.

2. The term *concrete theory* is thus a shortened form of the correct designation, *concrete universal theory.* While the term *concrete* has seemed to some unnecessarily industrial in tone, it recommends itself by its specificity and a robustness that makes it easy to remember. Alternatives that might be proposed, such as *middle-range political theory,* are so diffuse that they fail to provide the sharp definition I believe the method needs if it is to engage attention.

3. There are also penumbra works (e.g., Moe 1980; Popkin 1979), developing potentially concrete approaches but not strictly representative of the genre because they do not exemplify the full cluster of characteristics by which concrete theory has been defined. Nor was the "micropolitics" school (Kessel, Cole, and Seddig 1970) concrete in our sense, primarily because (as the editors themselves noted) no real theory was achieved (p. 7). The discipline has not wholly failed to recognize individual examples of concrete theory. Niemi (1986, 237) calls attention to the Jacobson and Kernell model and suggests that we need more of such theories. Almond (1988, 833) refers to Bates and North as among those who have softened the "hard Right" emphasis on short-run utility maximization through the use of "sociological" factors. Levi (1987, 685) emphasizes the institutional aspects of the work of both Przeworski and Sprague and North and Thomas.

4. I do not wish to suggest that situational analysis and concrete theory are by any means equivalent. Situational analysis includes a wide variety of nonpolitical topics (Gombrich on art, for instance), as well as all of microeconomic theory and Duverger's and Michels's Laws (Farr 1987, 56–60). It is undoubtedly too soon to sort this diversity into more orderly forms.

5. Potential areas of further application of concrete methods would include the interplay of decision making among high government officials (Birnbaum and Murray 1988), leadership and breakdown in revolutionary groups or parties, formal and informal interactions in legislative committees, military-government relations in developing nations, and so on. The theories discussed above also call for greater work on the cognitive description of decision-making elites, especially in respect to their idea systems. This cognitive aspect is particularly central to the work of Bates (1981); George and Smoke (1974); Miller and Jennings (1986); Przeworski and Sprague (1986); and Skowronek (1984).

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Forthcoming in December

The following articles, research notes, and controversies, have been tentatively scheduled for publication in the December 1990 issue:

Richard J. Arneson. "Is Work Special? Justice and the Distribution of Employment."

Christopher J. Duncan and Peter J. Steinberger. "Ruling: Guardians and Philosopher Kings: A Response." A controversy.

Robert S. Erikson and David W. Romero. "Candidate Equilibrium and the Behavioral Model of the Vote."

Richard L. Hall and Bernard Grofman. "The Committee Assignment Process and the Conditional Nature of Committee Bias."

George Klosko. "The Moral Force of Political Obligations."

Alex Mintz and Chi Huang. "The 'Peace Dividend': Better Late Than Never." A research note.

Peter C. Ordeshook and Emerson Niou. "Conflict Stability in Anarchic International Systems."

Todd Sandler and John Conybeare. "The Triple Entente and the Triple Alliance." A research note.

Terry Sullivan. "Bargaining with the President: A Simple Game and New Evidence."

Aristide Tessitore. "Making the City Safe for Philosophy: *Nicomachean Ethics,* Book X."

Erratum

"Black and Hispanic Socioeconomic and Political Competition," by Paula D. McClain and Albert K. Karnig, June 1990, pp. 535-545. On page 537, first column, line three should read "nonpoverty (86%) and employment (94%) . . ." On page 538, the legend to Table 1 should contain the following value: $^{***}p < .01$. On page 540, the legend to Table 2 should read: $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

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ADVANCES IN PUBLIC POLICY

Making Public Policy: A Hopeful View of American Government. By Steven Kelman. New York: Basic Books, 1987. 332p. $9.95.

Evidence, Argument, and Persuasion in the Policy Process. By Giandomenico Majone. New Haven: Yale University Press, 1989. 190p. $28.75 cloth, $14.75 paper.

Beyond Privatization: The Tools of Government Action. Edited by Lester M. Salamon. Washington: Urban Institute, 1989. $14.75 paper.

Cynical views of government have dominated both the public and academic perspectives during the last half of the twentieth century. The cynicism in the United States and other advanced democracies is directed more toward the failure of government to produce socially desired results than toward issues of participatory democracy. Political scientists, economists, and other policy scholars have contributed to the cynicism in several ways. The pluralist contention that pursuit of self-interest by elected leaders, agency officials, and citizens will somehow produce socially desirable results flies in the face of vast evidence of private interest politics, corruption, implementation failures, poorly designed programs, ineffectiveness, and unintended consequences. The contention by economists and public choice theorists that markets usually outperform governments and that governments perform better if organized more like markets is itself a cynical view that ignores the pervasive inequalities and injustices perpetrated by markets. The effort by social scientists to conduct policy-relevant research to document the policy choices' consequences for society and contribute to improved social conditions has been viewed as outside the mainstream of accepted social science practice on the one hand and as an exercise in futility on the other.

The cynical perspectives, then, rest on the contentions that there is no invisible hand guiding the pluralist world of policy toward socially desirable results, that the self-interested nature of human conduct destines government to be worse than markets and other private decision processes, and that the infusion of information linking policies with substantive effects on the public is either impossible or irrelevant to decisionmakers. The three books reviewed here take issue with these contentions and each in its own way makes a major contribution to the development of policy theories that are relevant for both the study and practice of policymaking.

The most ambitious of the three is Steven Kelman's textbooklike treatment of public policy. His thesis is simple: *public-spiritedness* is essential to understanding the operation of U.S. government and, even more importantly, to the continued success of democracy: "I argue that for the policy making process to work well, high levels of public spirit on the part of participants in the process are necessary. By 'public spirit' I mean an inclination to make an honest effort to achieve good public policy. This contrasts with the behavior of self-interested participants in the process, who do not ask what policy would be right overall, but rather simply what policy would be best for themselves." (p. 10)

Kelman's book is organized in two parts, the first focusing on institutional characteristics of the policy processes (Congress, presidency, bureaucracy, Supreme Court, and civil service), and the second on an evaluation of how well the process serves the public interest. Throughout both he delights in taking exception to much of the conventional wisdom of modern political science. In the discussion of Congress, for example, he argues that statutes are more important than the presidency in directing agency behavior even though much of the current empirical work on principal-agent theory ignores the role of statutes. He argues that members of Congress are frequently public-spirited, at least partly because voters, the media, and other members of Congress respect those who look after the public interest more than those who serve narrow interests.

His chapter on the bureaucracy pokes fun at those who have only recently rediscovered "the state" and who have promoted "new" insights that agency officials exercise discretion. Agency officials are viewed by Kelman as people who pursue policy agendas and support the mission of the agency as reflected in the statutes. Again, public-spiritedness is considered to be a prominant value orientation of public officials and essential to the production of good public policy.

The second part of the book attempts to specify the role of public-spiritedness in producing good public policy. He argues that good public policy will be produced if there is a significant level of public spirit in the system, and if there is a "reasonable ability to create the organizational capacity necessary to produce final government actions that resemble earlier political choices" (p. 208). Unfortunately, he then proceeds to define good public policy as policy produced by persons who are public-spirited, rather than suggesting operational definitions for the various standards he suggested in earlier parts of the book. He uses a different approach to critique pluralist and Madisonian democracies and market-oriented policy approaches, holding them responsible for demonstrating the social desirability of their results. Thus, he implicitly assumes the truth of his own argument but criticizes other approaches that also have assumed, rather than tested, the truth of their propositions.

Kelman's evaluation of democracy in the United States is that it works pretty well, mainly "because the edifice is held up by norms that prescribe public spirit; these norms work against powerful forces of self interest in human nature and can be very fragile" (p. 249). He argues that a cynical description of a political world based on self-interest can itself serve to undermine these norms and thus become a self-fulfilling prophecy.

Majone and Salamon reflect the same optimism as Kelman, although they focus on different ideas about what is needed to improve government performance and enhance democracy. Majone accepts the value of policy analysis in the production of socially desirable public policy and the furthering of democracy; but he contends that policy analysis needs to be enlarged to include such common practices as persuasion, rationalization, and advocacy, thereby bringing these practices under rational control. He writes: "If the purpose of policy analysis is not simply to find out what is a good or satisfactory policy but to ensure that the policy will actually be chosen and implemented, then traditional skills are not sufficient. The analyst must also learn rhetorical and dialectic skills—the ability to define a problem according to various points of view, to draw an argument from many different sources, to adapt the argument to the audience, and to educate public opinion" (p. xii).

In the early parts of the book, Majone makes his case for the importance of policy analysis to democracy. He notes that analysis should play a major role in identifying problems that are in need of policy action and that analysis should provide grounds for later justification and explanation of policies that are chosen. Policy analysis, the choice of policy tools, and evaluation research all must deal with the normative, as well as technical, aspects of public policy: "To decide, even to decide correctly, is never enough in politics. Decisions must be legitimated, accepted, and carried out" (p. 31). Analysis has an important role to play in this process.

In subsequent chapters Majone confronts several of the anticipated objections to his enlarged view of policy analysis and provides information on how the different types of policy analysis would be conducted. For example, he argues in chapter 3 that policy analysis is no different from other sciences, most of which no longer contend that scientific knowledge is proven knowledge but admit that it is tentative and open to refutation. He notes that many of the most important scientific theories have become accepted because of the persuasiveness of their rationales rather than the "truth" of their propositions. Majone envisions an active, advocacy approach to analysis and discusses feasibility studies, strategies for changing institutions, the role of analysis in the selection of policy instruments, and evaluation research. In contrast with some of the other new approaches to policy analysis, however, Majone does not advocate abandoning the technical, quantitative, or empirical aspects of analysis and replacing them with qualitative or intuitive approaches. His approach fits nicely with those who contend that policy research should serve policy decisions rather than focus on narrow testing of policy propositions.

Salamon's edited volume deals with the very important issue of policy design and the selection of policy tools that will produce desirable results. An emphasis on policy design and policy tools fills an important gap in the literature, which traditionally has taken the characteristics of the policy itself as givens rather than as phenomena to be studied. Policymakers are able to choose from among many different tools, including grants, direct loans, loan guarantees, standards, charges, contracting, vouchers, sanctions, incentives, educational and informational programs, and so forth. These tools are not value-neutral, nor are they equally effective in producing socially just outcomes and the perpetuation of democratic institutions.

The underlying assumption of the book is that policy can be improved through greater understanding of the variety of tools that are available, why policymakers prefer some tools to others, and why and how the tools impinge on target populations.

The case studies and essays in Salamon's book draw attention to the importance of policy tools and present some of their many dimensions. Unfortunately, most of the authors do not take a very broad approach and tend to focus on the structural and administrative characteristics of policy tools rather than on the normative and behavioral assumptions on which they rest or their potential for improving policy outcomes and social conditions.

Even though all three books make important contributions to the study of public policy, much remains to be done. Why do so many policy researchers and other political scientists continue to believe that self-interested actions dominate the political and administrative spheres? Perhaps the most important reason is that the assumption of self-interest is intuitively plausible and permits the development of apparently powerful deductive theory. On the other hand, surely it is time to acknowledge—as Kelman suggests—that empirical research reveals a much more complex view of decisionmaking, including the role of altruism and norms. It is time to accept an alternative theory of individual behavior that includes altruism, norms, and simple heuristics, as well as self-interest. An alternative theory will not be as parsimonious as self-interested utility maximization and will reduce the ability to engage in purely deductive theorizing; but it will enhance the ability to explain and account for the determinants, as well as the consequences, of public policy.

A second important issue is the reluctance of political scientists and other social scientists to conduct empirical research showing the impact of policy on social conditions and their hesitancy about using these results to inform policy debates. Majone's book makes progress toward legitimizing policy analysis as a form of social research, and Salamon's book is instructive about how the characteristics of policy might be incorporated into such analysis. But the deeper issues, here too, are tied to the atheoretical and noncumulative aspects of much of the policy research. Policies seem to be too complicated, too chaotic, and too unique to be worth studying. Many political scientists are too impatient to undertake the time-consuming and tedious work that would be involved in developing unified policy theory. Yet surely, this should be viewed as a challenge to the theory and methods of political science rather than a reason to eschew such an obviously large and important area of study. Advances will require the development of cross-policy theory that delineates the important dimensions of policy tools (such as their underlying behavioral, normative, and administrative assumptions), the decisionmaking of those who design and implement policies, the characteristics and motivations of target populations who are directly impinged on by the policy, and the contexts within which different results are observed.

ANNE L. SCHNEIDER

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Can the Government Govern? Edited by John E. Chubb and Paul E. Peterson. Washington: Brookings Institution, 1989. 339p. $29.95 cloth, $10.95 paper.

How can public policy be rendered more coherent and consistent when power is becoming ever more diffuse—within a fragmented Congress, an aimless bureaucracy, and an increasingly isolated executive? How can the public interest be better served when all of these power centers are growing ever more vulnerable to special interests? The authors of this thoughtful volume conclude, grimly, that without fundamental change in the structure of government, public policy cannot serve the public.

The specific impediments to good policymaking are by now well known: gridlock between an increasingly partisan Congress and the executive; the decline of institutional accountability within Congress as subcommittees have proliferated (now numbering more than 300) and staff memberships burgeoned; the expansion of the White House staff and concomitant isolation of the president from the bureaucracy; and the vast and amorphous growth of programs and agencies, often perversely structured so as not to be capable of doing what they were ostensibly designed to do and continually responsive to the parochial demands of specific legislators.

The virtue of this volume lies in the lucid connections, drawn by the authors, between these impediments and specific policy failures of recent years. John E. Chubb, for example, examines U.S. energy policy and finds it hobbled by an administrative structure predating the 20-year decline in domestic oil production. Special interests, combined with an inability (and unwillingness) of politicians and bureaucrats to alter how policies are made, have spawned decisions encouraging the consumption of foreign oil rather than spurring conservation or the pursuit of new energy sources.

David Yoffie finds a similar mismatch between how trade policies are devised and changes in the world economy that require a more strategic approach to trade. Here, again, a fragmented decisionmaking structure has made it difficult for administrators to set sensible priorities, and to avoid special interest demands for trade protection. As a result, protectionism has mounted, while politicians' attention has focused on domestic industries least able to compete in global markets.

Paul Peterson and Mark Rom complete the case studies by examining the failure of fiscal policymaking in recent years. With Congress and the executive blaming each other and neither willing to accept responsibility for hard choices, public spending has grown without corresponding growth in revenues, resulting in ballooning budget deficits.

The contributors are better at describing the structural problems than devising remedies. Kenneth Schepsle examines changes that have occurred in the structure of Congress in recent years, like the decline of seniority and the profusion of subcommittees and staff positions, and relates these to substantive failures to create coherent and consistent legislation. But he fails to suggest a convincing direction for reform. Chubb would like to free up energy prices, but does not tell us how this might be accomplished. Yoffee wants a more strategic trade policy, with better coordination between the branches but fails to plot the means for achieving it. Peterson and Rom want a balanced budget amendment; but quite apart from the dubious argument that such a device would lead to more responsible fiscal policy, they do not tell us how such an amendment might be implemented.

A recurring theme throughout the volume is that the most important battles in Washington are over decisionmaking structures, bureaucratic arrangements, procedures, and criteria, rather than over substance. Indeed, Washington "insiders"—repeat players in the policy game—well understand the importance of process. Terry Moe argues, convincingly, that administrative agencies are sometimes structurally unsuited for effective action precisely because interest groups and politicians have designed them to be ineffective. The public believes it is getting something that the insiders know will not be achieved.

These are not new problems or perspectives, of course, although they may appear to be more pressing than before. The progressivist debates leading up to civil service reform and to independent regulatory commissions mirrored many of these complaints, as did the arguments used, a half-century later, to justify the enlargement of White House staff offices and then, in the wake of Watergate, to achieve some measure of campaign finance reform. But each layer of reform seems only to have further complicated the process of policymaking; and the complications, in turn, seem to have further isolated policymakers from the broad public and render them ever more vulnerable to special interests. Such, at least, is this volume's perspective.

But if this were all there was to the story, one would be hard put to explain several dramatic instances during the past dozen years of public victories over special interests and shortsightedness—the abolition of many price and entry regulations for transportation and communications, comprehensive tax reform and (most recently) the closing of dozens of domestic military bases. Policymaking in each of these instances was dominated by coherent and consistent visions of the public interest; advanced by coalitions of legislators, administrators, public interest groups, academicians, and outside experts; and supported by the courts and the media.

Perhaps there is more cause for optimism than the contributors of this volume suggest. The formal structures of policymaking may be growing less capable of generating consistent and sensible policy, while the informal structures—what Hugh Heclo has termed, in a different context, "issues networks"—become ever more coherent and resiliant. Congress and the executive are losing the capacity to design policy for the long term; but not-for-profit foundations, academics, the courts, the media, and emerging networks of people who have previously served Congress or the executive are gaining. This volume would have been stronger—and more balanced—had it discussed these informal structures and focused on one or two recent policy successes.

ROBERT B. REICH

Harvard University

Balancing Acts: Technology, Finance, and the American Future. By James K. Galbraith. New York: Basic Books, 1989. 288p. $19.95.

This book presents the author's vision of the economic problems facing the United States, the reasons behind them, the choices before us, and the policies he thinks right. While written by a professional economist, the author has a wider audience in mind; and political scientists should find the book completely accessible.

Economists are not of one mind regarding how to evaluate the recent economic performance of the United States, where its weaknesses lie, or how to remedy them. In this book James Galbraith's focus is on macroeconomic performance, in particular slow growth of productivity and incomes and the large balance-of-trade deficit that he believes is unsustainable. He argues that the low rate of investment in new plant and equipment (which is part of the reason for slow growth) and the trade deficit are related to the deficit of the federal government, which he says must be reduced significantly. All of this is quite conventional macroeconomic argument. However, Galbraith is also concerned about the allocation of the investment increases and the reduction in the trade deficit that would go with a shrunken federal deficit.

He sees a choice between two different routes. One would be to shrink imports and expand U.S. capacity and performance in import-competing fields such as consumer electronics, automobiles, steel, and textiles. The other would be to expand capacity and increase exports in the production of high-technology products, which he sees as principally capital goods. He argues that if we go the former route, there is no way our living standards will not gravitate to the levels of the other countries producing those commodities. On the other hand, in Galbraith's view, the United States can continue to be—or reclaim its role as—the world's economic and technological leader if it pursues the high-technology high-export option.

How to do this? Closing the government's deficit is a prerequisite. However, Galbraith is no crude slasher of government spending. He stresses the importance of enhanced public funding on education, basic research, and economic infrastructure like roads and bridges. Writing before the Gorbachev revolution, Galbraith argues the need for tax increases to fund these investments and close the deficit. I suspect that he would now argue the use of the "peace bonus."

In any case, with a smaller deficit we could afford to loosen up monetary policy and reduce real interest rates. Galbraith advances the orthodox macroeconomic argument that this would at once increase U.S. investment and lower the price of the dollar and thus stimulate exports. He further advocates significant help for the Third World on their debt problem both for humanitarian reasons and to enable them to import more of our products.

All this is very mainline. Yet throughout the book Galbraith writes with a tone suggesting he is proposing something different from the standard sensible Democratic stock in trade.

In my view he is and he isn't. He does offer something different (how sensible or salable it is is another matter) in his proposals to change how private labor markets work and how the federal government adjusts to inflation. These proposals are motivated by his concern that the U.S. economy is presently too prone to inflation at high employment levels. He argues that either this or the policy response to inflation has been a major factor behind the poor U.S. economic performance of the last 15 years. I agree. The arguments and his proposals are too complex to go into here.

What gives the book its somewhat odd tone, combining a liberal orthodoxy of proposal with a rhetoric that seems to promise something more, or at least different, is his embrace of the objectives put forth by the exponents of an active industrial policy but rejection of any such policy. He likes high tech but doubts that the government is good picking winners. He advocates public investment in basic research and education; but as for investment in industrial research and development and new plant and equipment, he is in favor of letting the market decide. His tone is one of industrial policy bashing.

But it seems to me that Galbraith is mute exactly where the key "industrial policy" issues lie. Thus, consider the current brouhaha about semiconductors. As noted, Galbraith thinks we ought to favor high technology industries and says that government ought to fund basic research but not industrial research. By *basic research* does he mean to include or exclude National Science Foundation-funded university-based engineering research centers dedicated to advancing the technology of microelectronics? How about Sematech? High-definition TV? How about changing antitrust law to permit U.S. Memories? Galbraith gives no clue how he would come down on these issues.

All in all I like this book. Its policy prescription, however, is much more orthodox than the tone of the writing. And given that the author shares the objectives of the advocates of industrial policy, simply pooh-poohing their proposals will not do. The issues they point to are real and will not go away.

RICHARD R. NELSON

Columbia University

Day of Reckoning: The Consequences of American Economic Policy under Reagan and After. By Benjamin M. Friedman. New York: Random House, 1988. 324p. $19.95.

The New Competitors: How Foreign Investors Are Changing the U.S. Economy. By Norman Glickman and Douglas Woodward. New York: Basic Books, 1989. 384p. $19.95.

Making America Competitive: Policies for a Global Future. By Marcia Lynn Whicker and Raymond A. Moore. New York: Praeger, 1988. 216p.

Managing the American Economy, from Roosevelt to Reagan. By Nicolas Spulber. Bloomington: Indiana University Press, 1989. 172p.

The growth of the United States' national debt and its twin deficits—the trade and government budget deficits—seems matched these days only by the rising number of pages written on this topic. The present books deal with the problem of U.S. debt and deficits, examining the causes, consequences, and possible remedies for them. The authors view the persistent U.S. trade and budget deficits as the most critical issue for the United States today and as a sign of deeper problems that must be addressed. Inflation—once the central concern for policymakers and scholars in the 1970s and early 1980s—now takes second place to debt as an object of concern.

The authors (political scientists and economists) share three other key assumptions. First, they believe that government policy matters a great deal and that it can exert an ameliorative influence on economic outcomes. Broadly, all of the authors seem to be Keynesian liberals. For them, incorrect government policies, rather than government intervention per se, are the cause of U.S. economic problems. Their solution is, then, not less intervention but different and better policies. All of the books seek to contribute to debates about what the U.S. government should do next.

Second, the authors all implicitly or explicitly acknowledge the growing importance of international economic forces for the U.S. economy. No longer can U.S. domestic economic problems and policies be understood without reference to international economic pressures. The causes and consequences of the U.S. budget and trade deficits have international ramifications. Concerns about the competitiveness of U.S. industry and the impact of direct foreign investment reflect the new salience of the international environment. Finally, the authors reveal this tightening linkage between the international and domestic economy in their preoccupation with the deficits and another critical issue, the decline of U.S. power. For a variety of reasons, each tends to equate the U.S. deficits with a loss of U.S. control over its own and other's destinies.

Finally, the books approach the subject of debts and deficits primarily from an economic perspective. They focus on the economic causes and consequences of deficits and the policies associated with them. The political and sociological aspects are largely ignored. While the economics of deficits is a large and important subject, neglect of the politics and sociology surrounding it is a draw back. While illuminating the economic consequences of macroeconomic policies, the authors hardly touch on the political and social forces that produced these policy choices. This not only makes their analyses incomplete but also makes the political feasibility of their proposed solutions questionable.

Benjamin Friedman, in his well-written and provocative book, *Day of Reckoning*, focuses on the consequences of what he sees as the United States' number-one economic problem, the U.S. budget deficit and mounting national debt. In his opinion, the cause of this is Ronald Reagan's economic policies, in particular his tax cuts and defense-spending increases. While attributing this to Reagan and "self-deception" on the part of voters, he does not look any further into the political motivations for these policies. Was the public deceived by Reagan's claims, or did they actually want lower taxes and greater feelings of security? The answer to this question affects the central argument in Friedman's book. He claims that since citizens hold the basic moral principle that "men and women should work and eat, earn and spend, both privately and collectively, so that their children and their children's children would inherit a better world" (p. 4) and that we "want the level of government activity that we have" (p. 286), the budget deficit is immoral and should be reduced.

But neither of these propositions is obvious. In the 1980s, citizens have piled up debt privately (about two trillion dollars, almost the same size as the federal government debt), have led peaceful revolts against paying taxes (Proposition 13 in California), and have voted overwhelmingly for those who have proposed "less" government. The causes of our deficit have been just as political as economic. Deficits have been one way to satisfy unlimited and disparate political demands by different constituencies with limited resources, just as inflation was in the 1970s. By not analyzing the political sources of the deficit, Friedman also runs into problems when he proposes solutions for it.

Friedman identifies at least three interrelated problems due to the budget deficit: overconsumption, crowding out of private investment and its impact on future economic growth, and loss of international economic competitiveness and growing international indebtedness of the United States. He claims that "the effect of our new fiscal policy on our spending patterns is systematic overconsumption" (p. 41). The corollary of this is underinvestment, which in his view means that future growth will be impaired. Hence, as economic growth fails to keep up with interest costs, our children will face an evergrowing debt burden due to our overconsumption. Furthermore, the budget deficit "crowds out" private investment. Financing the deficit drives real interest rates up, which discourages investment. Friedman is certainly right on the first point: if we do not invest enough to keep economic growth outpacing the interest costs on our debt, our children will face a lower standard of living in the future. This is a big *if*, however. The interest costs themselves do not necessarily impose a burden on the economy, for 87% of them go right back into the economy as payments to U.S. government-bond-holders. (The other 13% goes to foreign holders of U.S. debt.) There is a redistribution occurring, but it is less from one generation to the next than from tax payers to government-bond-holders (i.e., from the middle class to the wealthy). The crowding out hypothesis also has a number of problems. In a growing economy or one with unused resources, government spending does not cause crowding out. Moreover, if government spending is used for public investment, it may be more productive than any marginal addition to private investment. Friedman's arguments about the effects of government deficits depend on a set of unstated assumptions that need to be examined.

Friedman's final point that the deficit promotes the loss of U.S. power internationally is also debatable. He claims, "Even if the sole economic effect of America's new fiscal policy were to cripple our growth, in time that wholly internal failure would constrain our international relations as well. But because we have also become a net debtor heavily dependent on foreign lenders, the implications are more serious" (p. 51). For him, the problem of underinvestment is compounded by the trade deficits, which the budget deficit also helps cause. The loss of our competitiveness due partially to underinvestment exacerbates the trade problem created by high U.S. interest rates. As the United States sucks in imports, foreigners earn dollars, which they in turn use to buy U.S. securities, real estate, factories, and so on. This foreign investment undermines our sovereignty and, as it turns the United States into a debtor, impairs our ability to resist protectionism and act as international lender of last resort. It is the loss of our net creditor position that most upsets Friedman: "World power and influence have historically accrued to creditor countries" (p. 76). The issue is, however, what would the United States like to do today that it can't because of its debtor status? Friedman cites the issue of Third World debt, suggesting that a U.S. "Marshall Plan" to relieve them of their debt is no longer possible given our financial straits (p. 82). But politically, would U.S. citizens ever approve of such a government "bailout" of the Third World? Calls for a Third World "Marshall Plan" since the 1950s have failed to be heeded in the United States. The United States has never pursued a massive foreign aid plan, the only exception being the Marshall Plan, which was developed under very special circumstances and given to highly developed Western Europe. The issue that needs to be addressed is, What does the deficit prevent the United States from doing internationally that it otherwise would want to do?

Friedman's remedies for our problems spring from his evaluation of the twin deficits' causes and consequences. He calls for increased taxes and a "significant easing of monetary policy" (p. 288). He sees increased taxes as a moral imperative and claims that "we need a tax increase up front, plainly in view, comprehensible to everyone. Americans want the services that the government provides. We can afford to pay for them. It is time to do so" (p. 288).

While economically reasonable, these prescriptions seem to lack political feasibility. Will the public willingly reduce its consumption? Do citizens believe that they should, and can afford to, pay new taxes? What will happen to politicians who propose and vote for such policies? Maybe citizens do not mind living with the deficit, given the alternatives.

Glickman and Woodward in *The New Competitors* explore the issue of how direct foreign investment, spurred in part by the deficits, is affecting the United States. Noting that such investment may have positive as well as negative consequences, they set out to evaluate its economic effects. As for the nature and size of direct foreign investment in the United States, they show that it is widely held among different advanced industrial countries and that its size is not large. (In the largest case, manufacturing, it accounts for only 7.8% of total employment [p. 45].) Its economic consequences, in their opinion, are not dire. While their analysis reveals that direct foreign investment does not create many jobs, it also does not destroy many. Their major argument is thus that state governments in the United States should not compete for direct foreign investment by giving these investors special deals. Foreign investment is not worth the effort: "Just as international investment in this country does not threaten us, it will not save us either" (p. 285). To deal with the internationalization of the United States economy, the authors recommend that the trade and budget deficits be reduced in much the same way that Friedman advocates. They also call for a program to collect more information on direct foreign investment and for one to promote U.S. "economic security." While ominous-sounding, the latter implies an industrial policy combined with a job security program. How politically feasible these programs are is questionable, but they may inform the next Democratic presidential candidate's agenda.

Whicker and Moore, in *Making America Competitive*, look at one of the issues that Friedman raises in connection with the budget deficit problem. Their concern is the United States' loss of international competitiveness. They, too, attribute U.S. trade problems to the budget deficits of the 1980s. Fiscal and monetary policies leading to the budget overruns promote trade deficits, which "are simultaneously symbols and products of declining American competitiveness" (p. 21). After this diagnosis, they point out other social and political problems that have contributed to our loss of competitiveness: a poor educational system, an alarmingly expensive health care system, income inequalities, high crime rates, poor management-labor relations, and lack of investment. In discussing these problems, the authors do not, however, compare the United States' record on these to other countries or to our own past record. Are these problems in the United States really worse than in other countries that are outcompeting us? Have these problems grown in magnitude over time so that they now constrain our productivity, or have they always been there? Finally, may not all of these problems, as well as our loss of competitiveness, spring from common *political* failings, which they have not discussed?

The authors here, as in the previous two books, tend to attribute competitiveness problems to macroeconomic factors (e.g., the budget deficit and fiscal and monetary policies). But much of our problem in this area may lie with microeconomic factors. The authors suggest this in their conclusion when they assert that the key to U.S. competitiveness lies in fostering cooperative relations among management and labor (p. 193). While the "corporatist" solution they propose has been seen by others as the key to growth and competitiveness, this is certainly not an undisputed fact. More attention to microeconomic factors, however, is clearly an undisputed need when searching for the sources of, and solutions to, the loss of the United States' international economic competitiveness.

Nicolas Spulber's book, *Managing the American Economy, from Roosevelt to Reagan*, addresses an issue implicit in the other three books reviewed. Spulber looks at the ideas of economists and their impact on U.S. economic policy from the Great Depression to the present. The book provides an interesting overview of the different debates among economists and of how politicians have absorbed ideas and constructed policies from these debates. He divides the period up into three eras: the 1930s to the 1950s, when getting out of the Great Depression and preventing further recessions or depressions dominated; the 1960s, when accelerated economic growth was key; and the 1970s and 1980s, when concern over stagflation led to new policies to reinvigorate the economy. The economic debates and new ideas introduced into policy by Roosevelt and Reagan, he claims, have had the greatest impact on the U.S. economy (p. 122). Most interestingly, Spulber argues that the stagflation of the 1970s undermined the prevailing belief in Keynesianism and helped promote monetarist ideas. Reagan's policies were informed by the emergence of supply-side and monetarist ideas in debates among economists; and, as we have seen, these policies produced major changes in the U.S. economy: ideas matter.

As Spulber concludes, "the historical record of economic policies from the Great Depression to the end of stagflation that we have presented in these pages shows the enormously creative imaginative and versatile contributions of economic theory" (p. 143). He defends the economics profession, suggesting that controversies within it do not mean that it is bankrupt. Rather, "the debates are in many respects an expression of the profession's involvement in the country's policy problems and the outward manifestation of its vitality and dynamism" (p. 144). The books reviewed here corroborate Spulber's conclusion. They reveal the creative and versatile contributions that economic analysis can bring to public policymaking.

The addition of political analysis to the discussions in these four books could, however, enhance their contributions. Such analysis would raise important questions about the causes and consequences of U.S. macroeconomic policies: What are the political sources of support behind the policies chosen? Can new political coalitions be created to support different policies? What are the political reasons for, and consequences of, increased direct foreign investment in the United States? What political and social institutions affect U.S. competitiveness? Can these institutions be altered to adopt new practices (e.g., corporatism or industrial planning) to enhance our competitiveness? What political and social forces influence the particular set of economic ideas that are chosen to guide policy? Answers to these questions are also essential to help the United States understand and remedy its current economic problems.

HELEN V. MILNER

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POLITICAL THEORY

Community without Unity: A Politics of Derridian Extravagance. By William Corlett. Durham, NC: Duke University Press, 1989. 261p. $37.50.

William Corlett's book is published in a series called Post-Contemporary Interventions, edited by Stanley Fish and Fredric Jameson. Most political theorists are finding it strain enough to keep up with postmodernism; and now, *postcontemporary?* What Corlett seems to mean by that is something like "Away with all those bad binary distinctions that have so bedeviled thinking in the West, such dualisms as freedom versus authority, anarchy versus order, inside versus outside, reason and sanity versus unreason and insanity, man versus woman! All of them are life-denying and thought-stopping. With the help especially of Foucault and Derrida we can now break out of these self-constructed prisons and together build a freer, gentler common life, a life in which the madman within each of us joins hands with the saneman within each of us and where each and all of us join together in relations of extravagant gift exchange." This is a vision of Merrymount, with no Governor Endicott waiting to crash in and close the dancing down.

The dualism Corlett chiefly addresses is "free agency and collective unity" (p. 32), commonly called *individual* versus *community*. He makes an excursion across some of the contemporary writing on community and shows how writer after writer gets trapped within these opposites, tries to compromise between them, and fails miserably in the attempt. Corlett argues that the failures are the inevitable result of bad conceptualization. The whole "problem" must be deconstructed à la Derrida; then more hopeful possibilities will emerge. Perhaps. But perhaps Marx was right when he said that on the whole consciousness reflects life, not the other way around. Perhaps the theoretical ambivalence is a reflection of our actual life conditions. Our theorists, like very many ordinary men and women, long for communion but not too much of it, long also for privacy and autonomy but not too much of it.

I am not sure Corlett's attack succeeds. But his is a brave and vanguard effort. Few political theorists have moved as vigorously as he has to welcome into the discourse of political theory the thought we associate with such names as Foucault, Derrida, and Bataille. I must say that Corlett's own ideas are too often hard to follow, due less, I think, to the complexity and depth of the thought than to the looseness and airiness of the writing. He also offers us little practical guidance toward his vision of "community without unity," reminding us in his own defense that deconstructionists do not offer programs.

JOHN H. SCHAAR

University of California, Santa Cruz

Social Contract, Free Ride: A Study of the Public Goods Problem. By Anthony de Jasay. Oxford: Clarendon, 1989. 256p. $49.95.

In this highly stimulating book Anthony de Jasay sets out to explain why "it is of the essence of social coexistence that a person can get more than he pays for, and also that he can be made to pay for more than he gets" (p. 1). The first step in his argument is to expose the ideology of the social contract: the contention that in view of the public goods dilemma, the social contract ultimately is rooted in individual preference. Two elements of the ideology in particular are singled out for scrutiny: the "facile notion" that only the sovereign state is capable of enforcing contracts and the postulate of the instrumental, nonautonomous state. With respect to the former, de Jasay first shows, with the help of examples taken from the history of medieval Europe, that enforcement of contracts without intervention by the state has been common practice in the past. Subsequently, he demonstrates by means of an analysis of four games that set successively steeper obstacles to cooperation (the Assurance Game, Rousseau's Stag Hunt, Rocking the Boat, and the Prisoner's Dilemma) that also in the state of nature certain coordination problems can be solved by means of enforceable contracts—although he admits that the solutions derived by this fact-deficient exercise appear as a whole to be "unsatisfactory, incomplete, partial or precarious" (p. 68).

As to the assumption that the state is no more than a mechanical *instrument*, de Jasay argues that the social contract, by which the state and its monopoly of force are established, will also contain a social choice clause, which will stipulate how in the future to take account of the preferences of the individuals who are party to the contract. He then proceeds to demonstrate that whatever the content of the social choice rule agreed on, it will lead to a gradual encroachment on the private domain by the public domain and will be about "relaxing or severing altogether the nexus between contribution and benefit" (p. 106).

Having thus established that it cannot be maintained that if the state "had not emerged from multiple historical causes, rational men would have chosen to erect it" (p. 116), de Jasay continues his argument with a discussion of the conditions under which in the state of nature a noncommanded solution of public goods dilemmas can be reconciled with the pursuit of individual narrow self-interest. His conclusions are not very surprising to those familiar with the game of Chicken. There is a presumption that individuals will voluntarily subscribe to the provision of a public good if they (1) do not have a dominating strategy and (2) prefer being the only contributor as opposed to having no one contribute. Whether or not these conditions will be met will be a function of the individual's estimation of the "productivity of publicness" and the "dilution factor" of the public good in question *and* of the size of the minimal contributing set and the number of individuals in the relevant group that will contribute.

In conclusion de Jasay argues that although noncommanded solutions to public goods dilemmas are possible in the state of nature, the demand for fairness—that is to say, the demand that free riders should be either forced to pay or forcibly excluded from consuming the public good—will eventually lead to the social contract. The social contract, accordingly, "is nothing more exalted than an anti-free-rider device" (p. 242). The sad irony of it all is that with the social contract and compulsory contribution, individuals will develop a revenge strategy for "getting one's own back," with the result that the actual effect of the social contract "is to open up an altogether new ground on which free-riding thrives with impunity" (p. 243).

Although I am wholly on the side of de Jasay when he tries to show that most public goods dilemmas can be solved without state intervention and that the games people play are more like Chicken than the Prisoner's Dilemma, I am afraid that I find his reasoning, however ingenious and illuminating at times, too loose to carry convinction. This goes especially for his "as if" accounts of successful cooperation in the perfect state of nature. It seems to me to be in complete contradiction to the classic assumptions of the perfect state of nature to assume, as de Jasay does, that individuals are, for instance, part of "overlapping groups defined . . . culturally, by occupation [and] income" (p. 189). In the classic state of nature the individuals are supposed to be completely on their own; accordingly, there cannot be any culture, division of labor, or wealth, not to speak of turnstiles to prevent too many individuals from leaving the minimal contributing set (p. 208)! Nevertheless, I think that thanks to the wealth of material contained in the book and the range of topics discussed in it, students of political theory may find the study of *Social Contract, Free Ride* well worth their while.

ROBERT H. LIESHOUT

University of Twente

Political Obligation in a Liberal State. By Steven M. DeLue. Albany: State University of New York Press, 1989. 179p. $39.50 cloth, $12.95 paper.

At the present time influential scholars argue that political obligations cannot be supported on the premises of liberal political theory. Proponents of this view include A. John Simmons, Joseph Raz, and Robert Paul Wolff. In *Political Obligation in a Liberal State,* Steven DeLue takes it for granted that this problem has been solved. Following the work of John Rawls and myself, he argues that political obligations can be established on the principle of fairness. He believes, however, that liberal societies must satisfy additional requirements before they can ground "strong" (as opposed to "weak") obligations to obey state authority.

According to DeLue, a strong obligation is comprised of a moral requirement both to obey the law in general and to limit opposition to specific laws to actions that will not damage the political system. An individual with a weak obligation will still recognize a requirement to obey the law but will not feel it necessary so to restrict opposition. The implication is that faced with the potentially destructive opposition of citizens with only weak obligations, the state could respond with force, thereby, in effect, treating citizens with weak obligations as if they had none. To avoid such undesirable consequences, the state must establish strong obligations for its citizens.

Chief among the additional requirements of strong obligations is the propagation of an "enlarged culture." DeLue believes that the citizenry of a modern liberal state is implacably divided over questions of political values. In order to generate strong obligations, the state must allow individuals to argue for and promote their particular values to criticize state policies. An "enlarged culture" is one that fosters the traditional liberal values of freedom of thought and discussion, which are in turn closely bound up with individual development. The connection between an enlarged culture and strong obligations lies in the fact that when a government actively supports free discussion and is responsive to the resulting intellectual consensus, individuals will be willing to limit their political opposition appropriately.

The bulk of DeLue's book is devoted to fleshing out the concept of an enlarged culture. He draws on themes in Kant's philosophy and Adam Smith's notion of the impartial spectator. He also discusses a variety of related themes, including the means through which an enlarged culture can be brought about and the kind of educational system one would demand. The emphasis on enlarged culture is great enough to have figured in the title of the book. Though chapter 1 is devoted to the question of political obligation as traditionally conceived and contains brief critical discussions of some familiar alternative views, along with the endorsement of a fairness position, it is not clear how these advance the overall argument.

In many ways *Political Obligation in a Liberal State* is an attractive book. DeLue pursues a reasonable path through a thicket of complex, controversial issues and draws on a substantial body of scholarship (though mainly by political theorists, as opposed to the voluminous—and more rigorous—work of philosophers). DeLue's analysis is characterized by an attractive eclecticism as he weaves discrete discussions of different scholars into a coherent whole, exemplifying the open, reasoned debate that he expounds.

But there are disadvantages to his approach. DeLue's strategy of arguing from the conclusions of other scholars allows him to avoid direct confrontation with many thorny issues. In general, he makes little effort to anticipate and defuse possible objections to his claims. The result is that little in the book is strikingly new or original. The importance of freedom of thought and discussion is of course a commonplace of liberal political theory. DeLue's most striking contribution is connecting this up with questions of political obligation. But the connections are not adequately explored. For instance, discussion of the crucial distinction between strong and weak obligations is perfunctory, as is DeLue's account of exactly how the opportunity to participate in open political debate generates a strong moral requirement to accept its outcome. Absent rigorous analysis of these and other crucial points, the importance of enlarged culture seems more asserted than demonstrated.

GEORGE KLOSKO

University of Virginia

Heidegger and Nazism. By Victor Farias. Edited by Joseph Margolis and Tom Rockmore. Translated by Paul Burrell and Gabriel R. Ricci. Philadelphia: Temple University Press, 1989. 349p. $29.95.

The hundredth anniversary of Heidegger's birth has been relatively quiet and part of the reason for it is Victor Farias' book. Originally published in French after he was first unable to obtain a German publisher, the present English edition incorporates a translation of the French edition with material that is being incorporated into the forthcoming German edition. Farias has, it is clear, made it once and for all impossible to maintain that Heidegger's association with National Socialism was only temporary and instrumental to the preservation of the German university. This view, which Heidegger propagated in his own defense after the war, now appears false in almost all of its aspects.

Farias' book is cast as an intellectual and political biography. He establishes that even before his 1933 appointment as rector of the University of Freiburg Heidegger was actively involved in the politics of the restructuring of German higher education along lines consonant with the policies of Roehm's SA, that is, with the most radical "populist" elements of National Socialism. Heidegger's resignation from the rectorship in 1934 appears not as a resistance to increasing Nazi influences but more as the act of a radical frustrated by the apparent victory of a more traditional or conservative vision of the university supported by, among others, Rudolf Hess. Finally, Farias reminds us of, and adds further important material to, what others (especially Guido Schneeberger and Hugo Ott) have shown us: Heidegger remained a member of the Nazi party until 1945 and until the end of his life maintained the importance of what he called (in *An Introduction to Metaphysics* 1935/1953) "the inner truth and greatness of National Socialism."

Beyond the demonstration that Heidegger was (some kind of) a National Socialist, Farias also makes two other claims. The first is that Heidegger's public and intellectual activities not only helped legitimate but also influenced National Socialism. The second and related claim is that there is a necessary or highly probable relationship between the form and content of Heidegger's thought and fascism.

On the first claim Farias is at least partially convincing. We know from Hannah Arendt that in his classes Heidegger made words come to life again, that the philosophically oriented youth of Germany felt that "one [could] perhaps learn to think" with him, that his reputation extended beyond the university ("Heidegger at Eighty," *New York Review of Books*, October 1971). It is less clear but still probably the case that Heidegger had an influence upon doctrinal struggles inside National Socialism, and it is one of the merits of this book that it makes us recall that Nazism was an idea as well as a movement.

I do not think that Farias proves his claim about the relation of Heidegger's thought to National Socialism. He often relies on "sounds like, looks like" arguments (e.g., parallel citations of Hitler and Heidegger) and rhetorical questions. So determined is he to undermine Heidegger's standing as a human being (Heidegger was Farias' teacher) that he all but ignores the criticisms Heidegger did make of Nazi biologism and racism and tends simply to amalgamate Heidegger's nondemocratic conservative political orientation with the worst aspects of fascism. But Farias does force the question of the relation upon us: even if this is not a brilliant book, it is one that cannot be avoided. Heidegger is arguably (with Wittgenstein) one of the two greatest philosophers of the century. How are we to read his philosophy, given the political facts that Farias now irreducibly enforces on us?

It will not finally do, I think, to agree with Richard Rorty that we can dismiss Heidegger's life as that of the "average Schwarzwalder redneck." (Much of the time Rorty wants to preserve liberal politics without its attendant epistemology; here he seems to want to preserve epistemology without an attendant politics.) As Philippe Lacoue-Labarthe argues in *La fiction du politique*, Heidegger's political choices in the early thirties were "neither an accident, nor an error."

If Heidegger's choices were not an error (indeed, in Rome in 1936 he defended them to his student Karl Loewith), what in his thought made them possible? The answer is complex and can only be hinted at here. Heidegger held that the highest form of freedom was to place oneself under the law of one's own essence, that is, to be transparent to the terms of one's own existence. By the early 1930s he had come to understand essence as given historically. History now revealed itself in Germany as the *Führerprinzip;* reason and knowledge must, to be under the laws of their own essence, no longer be thought of as autonomous but must yield to what in the "Rector's Address" Heidegger calls the "spiritual mission that forces the fate of the German people to bear the stamp of its own history."

Such a stance in Heidegger led Jacques Derrida to suggest (in *De l'esprit)* that Heidegger was committed to a metaphysical (although not biological) racism, as when Heidegger suggested that "when the French begin to philosophize they speak German." What is clear is that Heidegger understands true identity (of a person, of a people) as the transparent imitation and thus fulfillment of a form. The form of the German language, Heidegger asserts, is that which is the most transparent to thought; hence, the German people must be the source of a new form for the West, much as the ancient Greeks had been to Europe. Heidegger thought that in Nationalism Socialism he discerned in the terms of his thought the beginnings of a new form. The question that his affiliation with Nazism poses for us is thus the question of whether or not Nazism is something new in the world, an aberration (a liberal conclusion), one of the possible fulfillments of the Western form (a conclusion shared by a range of people from Popper to Polanyi to Marxists), or simply a German problem (George Mosse's conclusion). One reason, then, to engage Heidegger is that doing so will lead us to raise, and not forget, this question.

TRACY B. STRONG

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Toward a Liberalism. By Richard E. Flathman. Ithaca: Cornell University Press, 1989. 223p. $29.95 cloth, $10.95 paper.

This book is an important contribution to contemporary liberal theory. It is important (1) because Flathman's gift of patient and probing analysis has seldom been deployed to more illuminating advantage; (2) because he does much to restore teleological considerations to what I regard as their proper place; and (3) because he mitigates, in a manner both sensible and principled, the excesses and implausibilities of a great many well-known liberal theories.

One of the chapters of this book is subtitled "A Chastened View of Citizenship." The book as a whole might well be described as a sustained exercise in chastening. Flathman endeavors, with considerable success, to show that authority is neither unimpugnable nor dispensable; that neither "high [Rousseauian] citizenship" nor political "withdrawalsim" can overcome the perplexities of citizenship, linked as it is to the practice of authority; that rights cannot be justified without reference to consequentialist considerations, which may serve to qualify the rigor of rights; and that liberal theory is much more complexly related to liberal practice than the historically dominant tradition of liberal scientific empiricism has maintained.

As one might have anticipated, the two brooding presences in this book are Oakeshott and Wittgenstein. In the opening chapter they are made to join forces on behalf of a kind of nondogmatic skepticism that is shown to stand in a supportive relation to liberal concepts and practices. In so doing, Flathman by implication situates himself between *foundationalism* and *antifoundationalism* (as these terms are now understood). On the one hand, he rejects the Cartesian maxim that epistemology precedes, clears the way for, and marks in advance the limits of, substantive philosophy. On the other hand he rejects the easy assumption that practical philosophy can be "political, not metaphysical"; what we think about practice must at least cohere with what we think about much else.

While Oakeshott and Wittgenstein are yoked together to support this thesis, they are soon put asunder. Flathman turns Wittgenstein against Oakeshott's thesis that liberalism can be purely "adverbial," that is, can construct its institutions and conduct its affairs in a manner that wholly disregards, or is neutral toward, the disparate purposes of individuals. Liberalism is an agreement not only in concepts but also (as Wittgenstein more generally teaches us) in "judgments." And, as Flathman insists, "our agreement in these judgments constitutes the language of our politics" (p. 63; see also pp. 56–57, 91–95, 100).

This point may be generalized far beyond Oakeshott. In searching critiques of John Rawls and Bruce Ackerman, Flathman reveals the assumptions about individual and collective ends lurking behind the facade of neutrality. There can be neither a theory, nor an effective defense, of liberalism without a more-than-minimal conception of the good. In this connection, Flathman's own proposal, the *liberal principle*, deserves more consideration than it has received.

This argument has important implications. If every political community is purposive and rests on some agreement, the distinction between *an authority* and *in authority* cannot revolve around the distinction between substance and procedure. This Flathman explicitly acknowledges, indeed emphasizes (p. 56). What he does not state nearly as clearly is that neither does the line between *an authority* and *in authority* fully correspond to the line between human hierarchy and human equality. The reason is straightforward: whatever a community's shared judgments, principles, and purposes may be, some individuals will prove to be more able than others to promote them. For precisely the instrumental reasons to which Flathman repeatedly adverts, such individuals will have a claim (albeit not always compelling in the face of competing considerations) to a status that I (at least) cannot easily distinguish from *an authority.* Could it be that in the last analysis there is only one basic form of authority?

Space permits only a few concluding thoughts about this thought-provoking book. Flathman's liberal principle focuses on individual desires, interests, and purposes. It is not, however, made clear whether we are to use simple aggregation or something else as we move from individual to collective principles. Second, Flathman suggests that we move from the liberal principle to the justification of rights by identifying—and singling out for special protection—interests that are particularly "deep and vital" (p. 200). If these notions are to function as terms of distinction, we will need a more fully developed philosophical anthropology. Finally, as Flathman rightly insists, a core motivation of liberalism has been to protect the effective exercise of individual diversity against coercive interference. If "neutrality" cannot be sustained either in theory or in practice, how should we best conceive of the relationship between individual purposiveness and our liberal "agreement in judgments?"

WILLIAM A. GALSTON

University of Maryland, College Park

Private Terror/Public Life. By James M. Glass. Ithaca: Cornell University Press, 1989. 253p. $29.95.

James Glass stands between two worlds. One world is part of the long-civilized discourse of philosophy, letters, and science, the world of Sophocles, Hobbes, Kierkegaard, Freud, Sartre, Foucault, Derrida, Kafka, and Levi-Strauss. The second world is the insane asylum. Since the time of the Greeks, political life has been narrowly held. Only in recent times has the idea become even thinkable that all human beings in the political community had a right to, or a need for, political participation. Glass follows this thread straight to the Sheppard and Enoch Pratt Hospital in Towson, Maryland.

Glass discusses three individuals at length. David is "a highly articulate, intelligent young man" in his mid twenties, with a diagnosis of manic-depressive psychosis. His hold on political reality is tenuous, as he wanders between conventional intersubjectivity and a private world of terror where the world and his self dissolve. "There are times," he says, "when I feel like a continuum of 'its,' just particles, no identity, nothing substantial inside, everything floating haphazardly around in my mind. The world shifts when you're crazy. It pushes you in a thousand directions; the compass spins frantically, the Sirens work their way into your soul. . . . I dissolve into nothing, a faceless being becoming particles, molecules, atoms, and then finally air. Who I am becomes smaller and smaller until even my consciousness floats off into the universe" (pp. 29, 33).

Ruth is "an attractive woman of thirty and the mother of three children, [who was] highly regarded in her orthodox Jewish community," diagnosed as schizophrenic. The daughter of parents who had survived the Holocaust, she carries this reality as part of her deepest self. Glass tells us of her "feelings of being less than human, of being hated, reviled, and rejected by a persecuting and treacherous community. Her emotional world revolved around periodic feelings of terror, emptiness, and depersonalization; she experienced herself as empty, her body as dead; care and empathy, she believed, are deceits that disguise the deviousness of human intent and the malevolence of human motive" (p. 58). Finally, Julia is a third central character, though less completely drawn than the others. She "sought both a spiritual annihilation and a penance expressed through physical mutilation. She lived as if in a prison, locked up inside herself; she tortured her physical being" (p. 106).

We ordinarily transform such people into the cases of clinical psychology. Yet Glass gives their lives a richer sense within a broader context of political and social theory. Thus situated, he uses them to clarify the relationship between community and individual. Outside established boundaries, we see differently the meaning of political participation, the content of citizenship, the range of affinity. We gain a new understanding of identity and radical individual freedom through the eyes of those separated, alienated, and exiled from political society. Public and private space, natural innocence and violence, rationality and irrationality, gain new content.

This is a brave and stunning book. Its narrative description carries the power of research conducted with empathy and imagination. Its interpretation shows us other worlds, other realities, existing side-by-side with, and usually invisible to, our own. The hospital and the individuals in it are beyond normal politics and normal political science. Yet by exception and difference they frame, illuminate, and deepen our understanding of the meaning, variety, and significance of our own political, scientific, and individual lives.

FRANCIS A. BEER

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John Locke's Liberalism. By Ruth W. Grant. Chicago: University of Chicago Press, 1987. 220p. $24.95.

Ruth W. Grant's book is of the explication de texte genre. It is a careful, patient, disciplined examination of Locke's chief philosophical and political writings, thoroughly traditional in its aims and methods. The dissertation upon which the book is based received the APSA's Leo Strauss Award; and that is, of course, a tribute to its quality.

Grant's Locke is the consummate liberal and also a coherent, consistent philosophical reasoner. He succeeded as well as anyone can in squaring the liberal circle: how can a doctrine based on natural and equal individual rights and liberties ever support political community and legitimate political domination and authority? Many serious students of the problem have concluded that the trick cannot be done and that Locke's own attempt to do it is a hodgepodge of rationalism and empiricism, conventionalism and foundationalism, religion and science, inconsistent and muddled argumentation—a mess. Still other students, notably Richard Rorty, have argued that the very conception of political theory held by Locke is a mistake: political societies, even good ones, need no philosophical foundations; and philosophy can't provide them anyway.

Grant is persuaded that "Locke's work can be read as a coherent, orderly demonstration" (p. 9), a work meant to be read "as a series of deductions from clearly stated initial premises [and] argued as if it were a geometric proof" (p. 7). Locke *did* provide clear and coherent criteria for distinguishing legitimate authority from illegitimate power, and he *did* go a long and useful way toward showing how his criteria provide useful (though of course not complete) guidance in the conduct of practical political affairs. Locke, in her view, has provided a coherent account of the just society; an account fully consistent with his philosophical and epistemological construction in the *Essay Concerning Human Understanding*. Grant gives considerable attention to the *Essay* and argues that too many other writers on Locke have failed to see him as a whole because they gave it too little attention. Indeed, her treatment of the connections between the *Essay* and the *Two Treatises* is what gives this book its most distinctive accent within the huge body of commentary on Locke. It is doubtful whether her book will change the views of those who see Locke the muddler, Locke the defender of property, Locke the writer of coded secret messages, Locke the radical whig, Locke whose theories might condemn slavery but whose actions certainly did not—or a dozen other Lockes. Still, it is a solid and thoughtful effort, a thoroughly worthy addition to the Locke literature. That is no small merit; for where so many have already staked their claims to the ground, it is hard to make a plantation of one's own. Within the Locke literature Grant's approach to Locke bears most similarity to the works of, say, J. W. Gough, Raymond Polin, and Martin Seliger and is of the same high quality.

JOHN H. SCHAAR

University of California, Santa Cruz

Book Reviews: Political Theory

The Structural Transformation of the Public Sphere: An Inquiry into a Category of Bourgeois Society. By Jürgen Habermas. Translated by Thomas Burger. Cambridge: MIT Press, 1989. 301p. $35.00.

The New Conservatism: Cultural Criticism and the Historians' Debate. By Jürgen Habermas. Edited and translated by Shierry Weber Nicholsen. Cambridge: MIT Press, 1989. 270p. $22.50.

The Studies in Contemporary German Social Thought series, under Thomas McCarthy's editorship, have now reached an impressive forty-one volumes, six authored by Jürgen Habermas. With the translation and publication of the present two texts the English-speaking world has finally been presented with a way of framing Habermas' notable contributions of the last three decades from his earliest theoretical critique of society to his most recent and timely political disputes with his contemporaries. We are now in a position to judge the relationship between the young radical critic and the mature critical theorist.

First published in 1962 as the fourth volume in Wilhelm Hennis's Politica series, *The Structural Transformation of the Public Sphere* (*Strukturwandel der Öffentlichkeit*) represents Habermas' early attempt to come to terms with the bourgeois-liberal notion of publicness, or the public sphere, from the perspective of the critical Marxism of the Frankfurt school. Submitted for the "Habilitation" at Marburg, Habermas' text did more to secure his reputation on the Continent than any of the other early writings, including *Theorie und Praxis,* published a year later in the same series. It succeeded, minimally, in winning a place among those guideposts so useful to students writing dissertations, an importance concealed by the piecemeal and delayed translation of this part of Habermas' work.

As the subtitle suggests, the main theoretical and historical context for the public sphere's "structural transformation" is established by "civil" or "bourgeois" society *(bürgerliche Gesellschaft),* a category whose ambiguity in translation already reveals the problematic of Habermas' lifelong struggle with the competing languages of market-oriented liberalism and discourse-oriented critique. In this work the genealogy of the public sphere is traced in the bourgeois era (roughly Hobbes to Mill) through various linguistic, sociological, and historical modes of analysis, enlivened with illustrations from the defining culture of the salon, the press, letter writing, art, and the like. Compared with Richard Sennett's or Marshall Berman's work, Habermas' text is more characterized by philosophical reflection than cultural history. Readers will likely find the reflections dense, the historical vignettes a welcome respite.

Habermas proposes to view the bourgeois public sphere emerging on the basis of a market economy "in the tension-charged field between state and society" (p. 141) but paradoxically as a part of the private realm populated by private persons. Spatial metaphors predominate as the political and literary public sphere (*Öffentlichkeit*) arises in opposition to the sphere of "public authority." *Öffentliche Gewalt* ("public force") would be a better rendering, as it still requires legitimation. As for the paradox, we learn that it was smoothed over by the liberals, dissected by Hegel, and heightened to the point of contradiction by Marx. Today, Habermas suggests, modern developments have simply subverted the old public sphere, providing it with "feudal features" and "liberal fictions" that leave us asking whether it can be reconstituted on a new basis that will "set in motion a *critical* process of public communication" (p. 232).

This question may not be original, as a glance at John Dewey will show; nevertheless, it is central to much of Habermas' subsequent work. Indeed, the 14 essays and one interview from the nineteen-eighties brought together in *The New Conservatism* record a profound fidelity to the engagement with the public sphere. If anything, the import of Habermas' first encounter becomes clearer and more urgent in these essays. Two reasons are apparent. First, the dynamic transformation of the public sphere has continued with new social movements, the educational revolution, advances in electronic media, and in general a newly emergent constellation of culture and politics. Second, the question of the public sphere has shown itself more than ever directly connected to the prospects for democratization, dramatized so powerfully in world politics since this collection went to press. What Habermas diagnoses as the "new obscurity" in our *Zeitgeist*—Western culture's loss of confidence and bewilderment among the intellectuals—has been overtaken by a tempest that the critical theorist may well find both invigorating and troubling.

In Germany, furthermore, the struggle over the public sphere has taken a decisive turn. No doubt the most impassioned and significant essays of *The New Conservatism* are those written as contributions to the continuing discussion of the role of the intellectuals and the university, the work of Heidegger and Carl Schmitt, and especially the "historians' debate" that broke out shortly after Reagan's juggling act of Bitburg and Bergen-Belsen. Now at issue, as Habermas insists, is the public use of history and the integrity of the public sphere, the only sphere where crimes and damages from the past can ever be settled. For Habermas postwar history must never retreat to apologetics while hiding behind the protective shield of science. If collective memory is used to create a constructive national idea, it must be a "constitutional patriotism" firmly anchored in the political experience of a Federal Republic of Germany that "opened itself without reservation to the political culture of the West" (p. 227). That accomplishment, for Habermas, is his generation's greatest legacy.

But is critical history in jeopardy? Can the public sphere be renewed? Since October 1989 such questions have become even more insistent than before. The answers of the present are equivocal and divided. In their matter-of-factness Habermas' efforts may exemplify the kind of critical discourse the public sphere must now hope to sustain.

LAWRENCE A. SCAFF

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Politics, Innocence, and the Limits of Goodness. By Peter Johnson. London: Routledge, 1988. 283p. $35.00.

This engaging study serves to illuminate political morality by exploring, in literature as well as in philosophical texts, the nature of moral innocence. The exploration is finely tuned and well informed. An introductory chapter is followed by critical analyses of Greek and Kantian treatments of virtuous action and political morality. In the third chapter Machiavelli's views are interpreted, as are contemporary analyses of the "dirty hands" problem by such theorists as Walzer, Thomas Nagel, and Bernard Williams; and the vice of pride is thoughtfully dissected with the help of Shakespeare and Hume. In the next chapter the meaning and import of absolute morality—and absolute virtue—is presented. In chapter 5 we discover the author's philosophical leanings as he goes about interpreting Hegel, Nietzsche, and Arendt on innocence, morality, and politics. Chapter 6 commends literature as an antidote to the abstractions and generalizations inflicted by moral and political philosophers. Chapters 7–9 explore three literary texts for their bearings on the issues at hand. A tenth chapter defends the proposition that moral character, action, and outcome are not just contingently related but are instead inherently, and so inevitably, connected.

The author has given the subject matter the kind of scrutiny it requires. I cannot do justice to the book's plenitude here. Among the riches I count two central claims: that an adequate understanding of political morality requires paying attention to moral character or disposition and that certain kinds of moral character have no place in politics. Moral innocence exemplifies these claims. Inspection of this moral disposition renders more complete and concrete our understanding of morality and politics and the connections between the two; and it is a disposition that neither can nor should be sustained in the public realm.

The innocents to whom Johnson refers are not those who suffer politics or are corrupted by it; nor does he mean those merely ignorant of, or naive about politics. The innocent here are active agents who embody absolute virtue. Their virtue is absolute because unbounded (e.g., by duty), pure because valued for its own sake, and incorruptible. But their absolute adherence to absolute virtue is completely unreflective: they do not know why they value virtue; and they do not resist corruption through moral strength but, rather, are naturally immune to it. Their ignorance, then, is distinctive as well: it encompasses not merely lack of prudence and an inability to learn from experience but also, more centrally, ignorance of evil. Moral innocence does not, because it cannot, negotiate with evil.

Moral innocence also appears in different guises, a fact moral and political philosophers usually miss and almost never investigate. Indeed, that they generally fail to recognize the multiplicity of moral dispositions and the bearing of these on political morality is, as I have intimated, one of Johnson's central charges. Literature can, he argues, help here. Three texts are selected to help in the case of moral innocence: Shakespeare's *Henry VI*, Melville's *Billy Budd*, and Graham Greene's *Quiet American* (whose innocent is Pyle, a Harvard graduate committed to bringing democracy to Indochina). All three characters are active forces in the public realm, but Henry's moral innocence appears as weakness and vacillation, Billy's as mute, unschooled purity that can express itself publicly only through violence, and Pyle's as fanaticism, reflecting principles provided by the Harvard library and a determination rooted in the liberal conviction that goodwill and sound principle can determine the course of political life.

That each of these characters produce disaster in the political realm is not surprising: Henry ruins a kingdom, Billy kills a superior officer, and Pyle kills for "America." But why, precisely, are we unsurprised? Probably because we are not in this area Kantians; we are not surprised when good persons do bad things in politics because we do not believe that politics can be construed simply as a field of application for rules arrived at in morality. Probably, in other words, we believe in some sort of consequentialist realism instead. On this view morality and politics are in frequent conflict if not altogether divided because virtue—typically in the vague form of "conscience" or "principle"—often has to give way so that greater evils may be avoided or some good achieved. Politics is a tough business, a realm of tragedy and/or moral pragmatism. Good politicians know this and so compromise with evil on consequentialist grounds; but good men like Henry and Pyle do not and so produce bad results.

Drawing in part on Hegel and Arendt, Johnson argues that this is not right. The key is to recognize that "it is not an accident that the moral innocents in all three texts are murdered, executed, or assassinated" and that each is thereby justly rewarded (p. 245). Their morality is unfit for politics because in that essential and valuable realm their goodness becomes, necessarily and inevitably, evil. A mature morality recognizes this and so properly limits itself, as Captain Vere must recognize Billy's guilt and take responsibility for his execution. Politics is indeed a tough business; but it is so less because virtue has so often to be compromised than because absolute virtue has to be sacrificed. And politics is indeed an autonomous realm; but "whether moral considerations protect or threaten [it] depends on the moral disposition involved" (p. 245). Generalizations are unwelcome here.

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University of South Carolina

Surviving Power: Exercising It and Giving It Up. By Xandra Kayden. New York: Free Press, 1990. 216p. $22.95.

Fluctuating Fortunes: The Political Power of Business in America. By David Vogel. New York: Basic Books, 1989. 352p. $20.95.

Three Faces of Power. By Kenneth E. Boulding. Newbury Park, CA: Sage, 1989. 259p. $28.00.

Power is enormously complex. Rather than being a single phenomenon, it is a complicated set of phenomena—as these three books attest. Drawing on interviews, Xandra Kayden takes us "up close and personal" to consider what individuals experience both when they hold power and when they relinquish it, treating power as a matter of occupying positions of high responsibility. David Vogel examines the capacity of U.S. business to shape the terms of public debate and influence policy choice, viewing power in terms of group conflict. Kenneth Boulding provides the most wide-ranging treatment, sketching in broad strokes the interrelations of three forms of power, enlarging the usual notions of coercion, exchange, and persuasion into what he terms *threat power*, *economic power*, and *integrative power* and then applying them to interpersonal relations and organizations.

Political psychologists will find Kayden's book of special interest. Her concern is not what people do with power "but what it does to people: what it is like to get it and use it, and what happens when it is lost" (p. 3). Kayden's book concentrates on how the experience of power tests the character of people, but she points us toward other important lines of inquiry as well. In particular, Kayden suggests that holding power is a transforming experience, reshaping the very identity of those who occupy positions of responsibility. Being a powerholder, she shows, is different from being an advocate. Even among those who are ideologically oriented, Kayden argues, "many are likely to experience a shift in allegiance from commitment to ideology to commitment to personal involvement and being part of the process" (p. 162). What some see as a lack of principle is potentially a step toward a more inclusive sense of obligation. Kayden, however, talks only indirectly about such issues as how to distinguish responsible leadership from opportunistic powerholding and what, beyond individual character, encourages one tendency over the other. Still, *Surviving Power* greatly expands our understanding of the personal dimension of powerholding.

Vogel's book will primarily interest students of group politics and the legislative process. Despite the subtitle, it is not a comprehensive treatment of the power of business in the United States; for instance, it deals only in passing with various structural arguments. Vogel traces the changing character of legislative and interest group politics extraordinarily well. In doing so, he provides an informative account of how business resources have been mobilized to play the advocacy game.

Vogel deliberately steers away from advancing a general theory of the power of U.S. business and indicates that any conclusion is historically bound. His caution is understandable and even commendable, but his analysis is ultimately disappointing. Key issues go unexplored. For example, Vogel shows that organized labor has become ineffective as a counterforce to business; that role is now played by environmental and other public interest groups. Yet Vogel does not pursue the implications of this important change in group alignment. At no point does he say much about what the decline of organized labor means for the politics of the workplace or what the weakened position of labor in the Democratic coalition means for class relations. Thus, nothing is made of the point that at the very time the economy is undergoing a major transformation, the labor force faces greatly reduced access to the policy process.

Without pursuing the consequences of changes in either the place of organized labor or the nature of the Democratic party, Vogel is comfortable with his conclusion that the "privileged position" of business has declined. However, to bolster that argument, he shifts his ground from the domestic to the international arena. Most of his book is about the capacity of business to assert its interests in Washington; in short, it is a study in legislative influence. But the concluding chapter introduces the changing position of the United States in the world economy. The point is indeed an important one, but this means that there is an unresolved ambiguity in his analysis. From the 1960s on, Vogel argues, the legislative influence of business fluctuates with the performance of the economy—the poorer the performance, the greater the legislative influence of business. By not clarifying the different dimensions of business power and how they are related, Vogel is left with the seeming conclusion that the more battered U.S. business is by global economic conditions, the greater its domestic political clout. But he also maintains that global competition has reduced the power of U.S. business to act autonomously.

But exactly what is Vogel contending? That U.S. business can often call the shots on the home front but not worldwide? Though that is a reasonable conclusion, it is not backed up; Vogel provides no analysis of the role of U.S. business in global affairs nor does he say *what*, in an era of multinational corporations, should be considered U.S. business. Moreover, Vogel's view about the privileged position of business on the home front is not entirely clear. Passage of social regulation legislation shows that business fails the decisional test for dominance. But that is nothing new; the passage of the Wagner Act and other New Deal legislation in the 1930s had already revealed the decisional power weakness of business. What about other dimensions of power? Vogel skirts the argument that private control of investment provides business with a systemic advantage limiting the scope of overt political struggle. Thus, while *Fluctuating Fortunes* provides an informative account of the legislative influence of business, Vogel never fully addresses the political economy context within which that influence occurs.

Of the three authors, Kenneth Boulding confronts the complexity of power most directly. Even so, his analysis is done in general terms that do not help much in disentangling complicated problems like the one David Vogel takes on. Still, Boulding's book has a wealth of insights. Although many analysts treat power as a form of control or domination, Boulding has a more expansive understanding than that. While not inattentive to domination, his main concern is with the capacity to act—*power to* rather than *power over.*

Instead of a sustained, step-by-step argument, Boulding's book offers a unifying theme—that forms of power are connected to one another. In Boulding's view, no "face" of power is likely to be very effective by itself. For example, coercion is often exercised by a structure that itself is held together by an integrative force. Indeed, for Boulding, integrative power is especially important, and constructive problem solving in the modern world depends on giving integrative power its due: "A widespread recognition of the complexity of the power relationship, particularly the great importance of integrative power, is a necessary step toward the development of a world that can make decisions—and good decisions—about world systems, and at the same time preserve a wide distribution of power among groups, nations, and individuals to deal with those problems that are essentially individual or local" (p. 250).

These three books all make contributions to the understanding of politics, and their contributions are far richer than what I can convey here. None, however, offers a tight theory of even a segment of the universe of power. To some readers that may be a disappointment; they may be inclined to argue that there is little point in talking about such an elusive phenomenon as power. Yet each of these books confirms that power is an essential concept in the study of politics even though its complexity prevents its reduction to a form of neat theorization.

CLARENCE N. STONE

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Marx, Engels, and Liberal Democracy. By Michael Levin. New York: St. Martin's, 1989. 197p. $35.00.

This thoughtful and illuminating book, which explores the uneasy relationship between Marx and Engels on the one hand and liberal democracy on the other, is particularly meaningful in the light of recent developments in the Soviet Union and Eastern Europe and should be of great interest to students of Marx and Marxism. Levin argues that Marx and Engels were required to come to grips with liberal democracy, which was a political system only beginning to come into existence in their lifetime. As they developed their critique of capitalism and their understanding of communism, they also developed an understanding and critique of democracy, the liberal constitutional state, the role of workers and a workers' party in bourgeois society, and the value of parliamentary democracy before and after the communist revolution.

Because of complexities, ambiguities, differences, and shifts in positions, it is not always easy to evaluate the position of Marx or Engels on liberal democracy. Nonetheless, Levin argues that certain propositions can generally be affirmed. Marx and Engels endorsed certain aspects of liberal democracy, especially the concept of universal suffrage, popular (majority rule) government, and freedom of speech and press. They were adversely critical of the betrayals, deceptions, illusions, and flaws of bourgeois democracy, which they generally saw as a form of oppressive class rule. They recognized, however, that parliamentary liberal democracy (particularly if it included universal suffrage, endorsed majority rule, and upheld basic political freedoms) could be the means to supplant capitalism with communism; in this sense bourgeois liberal democracy could be the last step on the road to communism. However, they warned against compromise and against succumbing to a political philosophy of democratic reform, which would fall short of communism. The communist revolution—whether achieved by universal suffrage, peacefully, or by majority force and violence against a repressive minority regime—remained their unalterable goal. Marx and Engels, furthermore, sought to achieve—beyond bourgeois society—a more complete social and economic democracy. Two models in particular might be pursued immediately after the communist revolution: Paris Commune (democratic and decentralized) or the dictatorship of the proletariat (coercive and centralized). Levin argues that the dictatorship of the proletariat (although never adequately developed) seems to be the more likely model favored for the interim period between the revolution and the advent of mature communism. Finally, they believed that communism would transcend the conflicts—and thus the politics—of liberal democracy.

Levin maintains that some aspects of the Marx-Engels critique of liberal democracy are still relevant today. Nevertheless, although he is sometimes too harsh in his judgment of contemporary liberal democracy, Levin clearly sees that Marx and Engels were mistaken in holding that the working class would inevitably opt for communism. And although Levin holds (as he explores the Marxist legacy) that Lenin was forced by circumstances and not by Marxist theory to opt for a sustainable, undemocratic communist revolution rather than a democratic, unsustainable one. Levin is also clear in his recognition of the weaknesses of the position of Marx and Engels on democracy, social change, and communism. They had, for example, a manipulative attitude toward parliamentary democracy. They had "an unconvincing account of how to get from here to there" (p. 168). They had "a too optimistic theory of man and society." "They believed that such evils of their time as class exploitation, inequality and excessive division of labour could be overcome and replaced by a harmony in which conflict, and hence politics, could be superceded" (p. 168). Levin also appreciates that bourgeois democracy is "easy to criticise but difficult to improve upon" (p. 164).

Those interested in supplementary (and, occasionally, different) perspectives on Levin's important subject should consult Frank Cunningham's *Democratic Theory and Socialism* (1987) and Richard N. Hunt's two volumes, *The Political Ideals of Marx and Engels* (1974, 1984). Cunningham argues the case for the desirability and feasibility of democratic socialism. Hunt gives greater prominence to the democratic commitments of Marx and Engels and disagrees with Levin on the question of Marx's leaning toward a vanguard party. Overall, Levin's clearly written analysis helps us to understand better the historical development and continuing evolution of Marxism and—indirectly—the remarkable events occurring today in the Sovet Union and Eastern Europe.

NEAL RIEMER

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Sacralizing the Secular: The Renaissance Origins of Modernity. By Stephen A. McKnight. Baton Rouge: Louisiana State University Press, 1989. 131p. $25.00.

Hans Blumenberg's thesis on modernity has helped to stimulate a broad reevaluation of modern social and political arrangements and their relation to forms of knowledge and belief. Various disciplines have been implicated in this reassessment. In history, Pocock's thesis concerning the relationship of Italian republicanism to Anglo-American government; in social theory, Habermas' attempt to defend the project of modernity on neo-Kantian grounds; in political theory, Charles Taylor's attempt to recover political identities that might redeem humanity from the more nasty secularizing forces of modernity; and in "postmodern" thought, Derrida and Foucault's critical attempts to reassess representation and its relationship to identity—(as well as other urgent problems) might all be understood as responses to Blumenberg's notions about the legitimacy of the modern age.

Stephen McKnight challenges the basic premises of most revised assessments of modernity by claiming that modernity emerged in the Renaissance as a recovery of ancient wisdom. The importance of his argument is that rather than understand the motives of such Renaissance thinkers as Galileo in purely secular terms, he suggests that such thinkers placed one or another version of God (man as God, for instance) at the center of their efforts to liberate humanity from the false premises of knowledge that prevented humanity from seeing truth and controlling fate. For McKnight, secularizing thinkers are those who emphasize contingency and chance in their thought, while *sacralizers* are those who are most concerned with control and order. Among sacralizing thinkers McKnight includes Pico, Ficino, Bruno, and Campanella. McKnight also notes sacralizing tendencies in such thinkers as Bacon, Comte, and Marx.

McKnight's argument is bold and potentially exciting. From his premise one might be able to infer that the basic thrust of much of Enlightenment, as well as Renaissance, thought has been to sneak God back into the affairs of humankind through the backdoor. One cannot help but think of Nietzsche's assertion concerning the death of God and how difficult and important the consequences of that death are, taking centuries to absorb and resulting in different forms of destructive denial. In this sense, a careful tracing of residues of sacralization in the writings of Renaissance thinkers and their descendants would be an important contribution to debates about modernity.

Unfortunately, McKnight has not written such a book. *Sacralizing the Secular* only gestures toward evidence, making its arguments by assertion, ad hominem characterization, and innuendo. The very brevity of the book ensures that McKnight cannot successfully demonstrate his case in regard to any of the thinkers he examines. That in itself is not necessarily a fatal flaw if one is not making grandiose claims. But when an author insists that he is demonstrating "how the basic assumptions about modernity and secularization must be revised" (p. 110), one expects much more than one- and two-paragraph summaries of the arguments of the most important works of protagonists in the historical debate.

Throughout this slim volume McKnight characterizes what he calls "conventional treatments of modernity" (p. 109) without referring to the authors and the arguments that he is challenging. His various forays into the thickets of historical philosophy seem uninformed by most of the debate that has accompanied the emergence of Pocock's work, for instance (in fact, Pocock is not mentioned in this book at all). Nor is the work of Habermas, Taylor, Derrida, or Foucault ever discussed.

McKnight may well be safe in response if he asserts that some of these works are irrelevant to historiographic argument. But it may well be that the severe limits in historical scholarship that McKnight demonstrates are a consequence of his own desire to legitimate and delegitimate. A student of Eric Vögelin's, he seems determined to vindicate Vögelin's conception of historical progress, endorsing those in general agreement with Vögelin's approach and ignoring or dismissing those in disagreement. Since those who are ignored include some of the most important thinkers of the day, McKnight's book is bound to do no more than preach to the already saved.

THOMAS L. DUMM

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The Nature of Politics. By Roger Masters. New Haven: Yale University Press, 1989. 298p. $25.00.

For more than one hundred years biology has significantly increased our knowledge of human behavior, and such advances in knowledge are likely to continue for decades. But since the time of Locke and Rousseau, biology has been separated from the social sciences. It is time for the two to be reintegrated, in particular for biology to be joined with the science of political behavior. In *The Nature of Politics* Roger Masters undertakes to speed the reintegration process. Is he successful?

Before answering this question, a few words about biology and the problems any attempted reintegration must face. Since Darwin biologists have gradually put together a compelling body of evidence and theory dealing with such issues as species similarities, selected traits (behaviors), genetic loading for behavior, and the molecular-neurophysiological mechanisms of behavior. This evidence and theory has led to critical changes in our views of human nature: we have learned that much of our behavior is strongly predisposed by our genetic makeup, that given certain conditions specific behaviors are highly likely, and that much of our behavior occurs in predictable ways independent of conscious attempts to behave otherwise. For example, it is not easy to forget an unreciprocated favor, however much one might try. The behaviors in question—on which Masters focuses (e.g., altruism, bonding, aggression, etc.)—are at the heart of everyday human interactions and decisions and thus at the core of political behavior. Going about integrating these findings into a field as broad and as diverse as political science would seem an overwhelming, if not impossible, task. In effect, an integrator must attempt to identify critical biological insights and frame them in such a way that they can be understood and assimilated into an already-existing body of thought.

Masters builds his case carefully and from the ground up. Part 1 addresses animal behavior and the factual and theoretical bases for human altruism. Despite the reports of our newspapers and television stations, the vast majority of human interactions are based on some type of altruism and interdependence. Moreover, there are very specific "rules" that guide such behavior. Part 2 elaborates the ideas from part 1, places them in a social context, and analyzes their relationships to bonding, aggression, flight, language, and cultural change. Part 3, "Politics and the State," further elaborates on the topics of genes, environment, and development; moves to a biological definition of politics; and finally comes to a concept of the "nature of the state," which is explained not only in terms of cooperation but also in terms of potentially conflicting yet stable behavior strategies. Part 4 addresses the question, Why bureaucracy? and closes with a brief review of the biology of social participation. Is the book successful?

The answer is *no* and *yes*. *No* because *The Nature of Politics* will not completely satisfy everyone. Many political scientists will argue that the author should have emphasized other issues or ideas. There are simply too many opinions about what political science is and what it is not to expect this book to achieve universal acclaim among the author's peers. Biologists will argue that he should have emphasized other biological issues or ideas. Biologists, too, have their spread of opinions (I have mine, sometimes in disagreement with Masters). Moreover, the fields of nature (biology) and politics are too diverse to define and integrate in three hundred pages, no matter how tightly written. *Yes*, instructively, because Masters has wedged open a door that can no longer be shut. Biology will not go away, and the science of political behavior is unlikely to survive without it. Biology-initiated insights into political behavior, such as the nature of cooperation, the basis of cooperation in the state, and the pervasiveness of nepotism, ostracism, xenophobia, deception, competitiveness, and the desire for power and control—all have strong roots in the biology of our species. This, of course, is what the field of biopolitics has been advocating for two decades (a point Masters acknowledges); yet few would listen. *The Nature of Politics* will make it harder not to listen. Masters has been successful.

I view this book as a major landmark on the road to developing a conceptual framework in which to interpret political behavior more precisely and critically—indeed, it is hard to imagine how any book dealing with political behavior during the next decade can be written without coming to terms with Masters's views. The author is at home in political science, philosophy, history, and much of biology. He guides the reader through a mass of complex ideas and findings with exceptional ease—the reader can know very little biology, history, or philosophy and comprehend the text. In short, this is a superb effort of thought and integration and, I would argue, an essential book for any serious political scientist, philosopher, or biologist who desires to enter the public domain.

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Gandhi's Political Philosophy: A Critical Examination. By Bhikhu Parekh. Notre Dame, IN: University of Notre Dame Press, 1989. 248p. $26.95.

Scholarly studies of Mohandas K. Gandhi usually fall into two distinct categories: historical narratives of his life and leadership and theoretical analyses of his thought. A prototype of the former is Judith Brown's work on Gandhi, including her new biography; a prototype of the latter is Raghavan Iyer's *The Moral and Political Thought of Mahatma Gandhi* (1973). Exceptional studies, such as those by Joan Bondurant or Susanne Rudolph, manage to cross these categories by coming up with stimulating combinations of historical and philosophical analysis. (But such studies are rarely attempted and even more rarely achieved).

Bhikhu Parekh's book is strictly a philosophical examination. Its purpose is not historical enquiry, so we are not given documentation of the raj's response to Gandhi that Brown gives so well. Parekh attempts, rather, to state systematically Gandhi's major ideas and his contribution to political theory. Gandhi's political philosophy is viewed as a substantial response to the intellectual domination exercised by Western political thought: "He was one of the first non-Western thinkers of the modern age to develop a political theory grounded in the unique experiences and articulated in terms of the indigenous philosophical vocabulary of his country. He conceptualized political life in an original and stimulating manner, placing new questions on the political agenda and offering new ways of tackling the old" (p. 3).

Parekh's analysis is skillful and perceptive: based almost entirely on Gandhi's writings, it illuminates in an admirably cogent and clear style the cohesiveness of Gandhi's thought. Gandhi's critique of modern civilization and defense of Indian civilization are rightly set forth as the starting point of his philosophy. Then his theories of religion and politics, the state and satyagraha, are seen as logically following from that initial critique in *Hind Swaraj.*

The signal strength of this book, therefore, lies in its systematic presentation of Gandhi's originality as a political thinker. It does not emulate the broad comparative analysis of Gandhi with Western theorists found in Iyer's longer study. However, its forceful criticisms of Gandhi's thought are uncommon in works of this kind, especially the incisive remarks on the limitations in the theory of satyagraha (pp. 166-70) and on how Gandhi's concept of Indian civilization blocked a full understanding of Hindu-Muslim conflict (pp. 185-94).

Neither Gandhi nor any of his interpreters claim that he was a systematic philosopher. Attempts to systematize his ideas, begun by Joan Bondurant, are problematic. The astonishing feature of this book is that in comparison with the legion of other works on the same subject, it has formulated Gandhi's thought so neatly while avoiding most of the pitfalls. There are masterful expositions of Gandhi's theories of "spiritual politics" and how they involved a reformulation of the traditional concept of moksha (pp. 95-109); of the evolution of Gandhi's concept of the state and how this idea in particular posed difficulties reconciling theory to practice (pp. 110-41); and of the relationship of the philosophy of satyagraha to his ideas of violence and power (pp. 147-55). The concluding "critical appreciation" manages to show how despite Gandhi's glaring failures as a reformer, "the unity of his thought and action became a powerful force of national inspiration" (p. 212).

The shortcomings of Parekh's book are minor compared with its accomplishments. Any effort at systematizing Gandhi's thought must infer a great deal from his writings. Although this particular work has the decided virtue of using Gandhi's own words often, there are nevertheless repeated instances where Parekh says, "Gandhi argues . . ." or "Gandhi observed . . ." and the subsequent inferences are undocumented or dubious. Moreover, one may take particular issue with how Parekh exaggerates the degree of rationalism in Gandhi's theory (pp. 164-66) and deemphasizes the extent of his nationalism (pp. 3, 194). It is incorrect to say that Gandhi "rarely used the term 'nation' except when forced to do so by such antagonists as Jinnah" or that he only "occasionally used the term 'nationalism' " and then only to show that he "conceptualised the Indian struggle for independence in a non-nationalist and non-national language" (p. 194). This is taking the argument for Gandhi's originality too far. In fact, he repeatedly used the word *nation* and his idea of nationalism was heavily indebted to Western thought.

This book offers a persuasive and powerful statement of Gandhi's significance as a political theorist. Here, as in his other writings on Gandhi's theory (especially his article in *Political Discourse* [1987], which he edited with Thomas Pantham), Parekh has helped increase our awareness of Gandhi as among the very few original political theorists of the twentieth century.

DENNIS DALTON

Columbia University

Fleeing the Iron Cage: Culture, Politics, and Modernity in the Thought of Max Weber. By Lawrence A. Scaff. Berkeley: University of California Press, 1989. 279p. $35.00.

The Political and Social Theory of Max Weber: Collected Essays. By Wolfgang J. Mommsen. Chicago: University of Chicago Press, 1989. 226p. $39.95.

The work of any great thinker is subject to interpretation, reinterpretation and, not infrequently, misinterpretation. This is perhaps especially evident in the case of thinkers long dead—Plato, More, Machiavelli, even Marx—but it is no less true of more modern (and ostensibly more accessible) figures such as Freud and Weber. Both of the latter lived in our own century, after all, and their presence is almost palpable. Each somehow feels familiar or, in the jargon of our age, "user-friendly." But, as Nietzsche warned, it is dangerous to try to domesticate a great thinker (he was referring to himself); for they do not make good house pets or even good guests. On the contrary, they are apt to soil the carpet, insult the host, and offend the other guests. A truly great thinker, Nietzsche averred, offends gods and men alike by questioning the conventional pieties and refusing to be tamed.

Heedless of Nietzsche's warning, U.S. social scientists have for some time tried to tame and domesticate Max Weber. Until quite recently, the Weber we knew—or thought we knew—was the good, gray proponent of objectivity and value-neutrality and the creative coiner of new concepts in the social science lexicon, including *charismatic leadership, instrumental rationality, Protestant ethic, bureaucratic rationalization,* and the rest. This was the domesticated Weber introduced to U.S. audience by Talcott Parsons's *Structure of Social Action* (1937) and his selections from Weber's *Theory of Social and Economic Organization* (1947), Edward Shils's *Methodology of the Social Sciences* (1948), Reinhard Bendix's *Max Weber: An Intellectual Portrait* (1962), and a number of other studies. Despite their differences, all were alike in making Max Weber into a respectable social scientist, in part by downplaying the importance of the intellectual and political context in which Weber worked. If Weber could be tamed only by taking him out of his historical and political context, placing him back in that context might permit us to see a different, and altogether more dangerous and interesting, social theorist. That, at any rate, is the historiographical claim connecting the two present books.

In attempting to find "the man we have not known, the thinker we have missed in the rush to establish the conceptual boundaries and methodological foundations of the social sciences," Lawrence Scaff in *Fleeing the Iron Cage,* approaches Weber "in his own times, through his own fin-de-siècle culture, his own language, problems, and intellectual experience" (pp. x–xi). The effort yields remarkable results. Scaff reveals a Weber who might be described less as a sociologist than as a Promethean—or better, perhaps, a Faustian—critic of modern culture and politics, whose very vocation drove him to the edge of madness. The "iron cage" and the "polar night of icy hardness and darkness" were, for Weber, more than decorative metaphors. They reveal his deeply felt despair over the "fate" of mankind in the modern age. This theme has been sounded before by, for example, Arthur Mitzman in *The Iron Cage* (1970). But Scaff has a better feel for, and a surer sense of, the intellectual and political climate in which Weber worked. *Fleeing the Iron Cage* is less a psychobiography than an anatomy of our age, with Weber as guide, diagnostician, and subject. Carefully crafted and beautifully written, Scaff's is the best book about Weber yet written in English.

No one has done more to distinguish what Scaff calls "the myth of Heidelberg" from the historically situated man and thinker than Wolfgang J. Mommsen, the very able and learned editor of Weber's collected works. Admirers of Mommsen's richly historical and path-breaking *Max Weber und die deutsche Politik* (1974) may, however, be a bit disappointed with his more recent book, *The Political and Social Theory of Max Weber.* It is actually a collection of lectures and essays cobbled together and published under a somewhat misleading title (misleading in that the *the* leads one to expect that this is a systematic exposition of Weber's theory, which it is not). This wide-ranging collection includes essays on the tension between political commitment and scholarly detachment, Weber's "dialogue" with Marx and relationship with Roberto Michels and other contemporaries, "rationalization" and "myth," the modern reception of Weber's thought, and other topics. But while each of the eleven essays is interesting and important in its own right, the whole is only the sum of its parts. Nor has Mommsen been consistently well served by his translators (e.g., "Max Weber was extremely irritated by the political arrangements in Imperial Germany" [p. 10], which is a bit like saying that Marx found capitalism not quite to his taste). Minor irritations aside, Mommsen's book is clearly worthy of close and careful study by students of Weber and modern social theory.

Taken together, these two studies attest to the importance of resituating Weber's thought in its own time and place as a prelude to understanding its bearing upon our own.

TERENCE BALL

University of Minnesota

Beyond Optimizing: A Study of Rational Choice. By Michael Slote. Cambridge: Harvard University Press, 1989. 192p. $22.50.

Michael Slote moves from a standard resolution of a bothersome problem in moral theory to what he proposes as a major innovation in the foundations of rational choice. The moral problem is that many theories that make beneficent actions morally right (as, for example, utilitarianism does) seem to demand more than we can expect of normal humans. If I can rescue two people from a burning building by charging into the flames, beneficence seems to require that I do so even though I am sure to suffer grievous harm. If we suppose most people would not brave the flames, we must suppose most people are inherently immoral. Somehow, this seems too heavy a charge to level against most people. Many moral theorists suppose that while it is good to charge into the flames in such a case, it is not morally required. Such action is supererogatory—beyond duty (literally, "beyond what is asked" by moral theory).

Slote's earlier book, *Common-Sense Morality and Consequentialism* (1985) argued that this moral-face-saving conclusion could be reached by a principle of rational choice. That principle he labeled *satisficing* after Herbert Simon's now-standard notion. Slote's use of the label was badly misleading. In Simon's vocabulary satisficing is a device for dealing with lack of information. *To satisfice* is to settle now on the best outcome one has found rather than continue to search for a better outcome. The alternative, continuing to search, may involve such great search costs that the combination of the better outcome less the costs incurred in finding it seems less good than the best of the presently available outcomes. Moreover, one might never find a better outcome. But Slote's satisficing allowed one to select a less-than-optimal action or outcome from a set in which there is a *known* better action or outcome. Slote supposed that if I faced a choice between *x* and *y* and I preferred *x* to *y* ceteris paribus, I might nevertheless rationally satisfice and choose *y*. Hence, the moral theorist who thought it best that I charge into the flames could also think it morally rational of me to satisfice and choose the less-good outcome in which I stand and watch you and your friend be immolated in the building.

Beyond Optimizing is largely a recasting of the earlier book, now with the emphasis changed to focus not on moral problems of supererogation but on problems in individual rationality. Slote likens his version of satisficing—of deliberately taking less than the best—to the virtue of moderation. He claims that it is a commonsense notion, that is, one we all know if we just think about it. To buttress this claim, he gives most of his space to arguing for various reasons one might satisfice, in his sense. He illustrates his arguments with numerous examples, many of which are very odd, including several that are lunatic science fiction or fairy-tale choices (e.g., pp. 14–15, 110–15) but none of which squares with my apparently uncommon sense.

Readers familiar with the traditional notion of satisficing may suspect that Slote has simply misunderstood the issue. When explicating his notion, he blurs the distinction between his and Simon's concept in discussing a standard example of satisficing behavior: settling on an adequate price or offer for one's house rather than holding out for an even better return (pp. 8–10). This is a clear instance of normal behavior that makes full sense under the notion of satisficing as balancing unknown information costs against potential benefits rather than as deliberately choosing a lesser over a greater good. However, as is clearer in his earlier book, Slote fully understands the difference between his satisficing and Simon's. Slote's argument cannot be dismissed as a verbal misunderstanding. He genuinely insists on the rationality of acting on his own notion, which many readers may sooner call *beneath optimizing* than *beyond optimizing*.

How does Slote's house seller reason? Slote says, "His desires, his needs, are moderate, and perhaps knowing this about himself, he may not be particularly interested in doing better for himself than he is likely to do by selling at a merely satisfactory price. If someone pointed out that it would be better for him to get more money, he would reply, not by disagreeing, but by pointing out that for him at least a good enough price is good enough" (pp. 9–10). It is hard to know quite what this house seller thinks. A typical social scientist is likely to wonder whether there is something hidden away in the ceteris paribus clause that is being glossed over here. Suppose this house seller is Slote, who is the first and maybe the only satisficer in his sense. If Slote were invited to take more for his house, would he accept it? No strings attached, no incident costs, no burdens of any kind, merely more versus less. At some point, many of us might finally say *no* if, no matter how insistent the buyer, we could not believe the ceteris paribus clause. Or we might even begin to think, in a corollary to the Bauhaus slogan, that at some point more is finally less. If I fell into too much money, my life-style might change in ways I would not like, so much, indeed, that I might think myself no longer me—I might even become Donald Trump. Slote allows that these are possibilities in some cases, but he insists that there are also cases in which we choose to "just say *no.*" He also wants us to say it's rational to "just say *no.*"

Slote thinks he is on to an important and very good new idea that merits maximal exposure in several articles and now in a second book. If the idea is that good, of course, then on the standard view of optimizing rationality it does merit such repetition. The idea, however, is that this standard view is inferior to a view that we should rationally settle for less than maximum returns or effects. On Slote's view, one might suppose he should have settled on presenting these views only once. On the standard view, one might think that more now threatens to become less.

RUSSELL HARDIN

University of Chicago

Political Hermeneutics: The Early Thinking of Hans-Georg Gadamer. By Robert R. Sullivan. University Park: Pennsylvania State University Press, 1989. 206p. $22.95.

Political theory at its best represents one side of a useful dialogue between philosophy and political science. Students of politics may thus be attracted to "political hermeneutics." Robert S. Sullivan's explication of the work of Hans-Georg Gadamer would appear to be a welcome source of knowledge to political scientists interested in how contemporary philosophy can contribute to the understanding of politics within the discipline of political science.

The concept of "discourse rationality" fully developed in Gadamer's *Truth and Method* (1975) might well provide us a new approach to the study of politics. Yet Sullivan himself directs the reader away from this approach: "Philosophical hermeneutics . . . is not political theory, and it has had limited impact on contemporary political theorists" (p. 7). Rather, it represents a philosophical alternative to the analytical tradition of "the so-called continental school of philosophy" in the areas of literature and the arts, the philosophy of science, and even the philosophy of law. Nevertheless, perhaps students of politics should pay more attention to this position. Sullivan prefers to examine the early, rather obscure works of Gadamer. The most explicit message for students of politics is that Gadamer's early works "remind us that the study of politics before the advent of political science in the seventeenth century was a distinctly humanistic concern of transmitting and renewing Western values" (p. 16). This is hardly a surprising or enlightening insight for contemporary students of political theory.

Sullivan presents an interesting discussion of the placement of Gadamer in early-twentieth-century German philosophy and the *Bildungs* tradition and spells out in detail how his works, beginning with his doctoral dissertations of 1922 and continuing with his work through 1934 fit within this context. For the student of German philosophy this is an interesting and sometimes enlightening exposition. Some insight is provided about how all of this relates to Nazism; although one is left rather disappointed with the ultimate conclusion that "[if] Gadamer did have a flirt with Nazism, it can only be accounted for in terms of the career ambitions of a young German academic" (p. 181). It is nice to know that Gadamer was less of a Nazi than "his mentor Heidegger" (p. 177) and that this had some relation to Gadamer's work with other German intellectuals. Yet ultimately one might wish for more serious consideration of the implications of the fact that Gadamer "remained politically neutral and had this neutrality certified after the war when the Soviet occupation authorities accepted him as the first postwar rector of the Leipzig University" (p. 177), especially when we find that one of the more significant messages of Gadamer's extensive work on Plato is that "politics, which conventionally and at its democratic best is an external dialogue with another, serves as a constitutional principle for the construction of the internal thinking soul of the philosopher" (p. 188).

Yet the bulk of the book does offer some insight for students of politics, or at least students of political theory. Sullivan's analysis of Gadamer's thought on Plato suggests that students of classical thought should read what Gadamer has to say in his early work. Especially intriguing is the discussion of Plato's view on the banning of the poets. The notion that Plato is interested in establishing dialogue rather than in substantive or "grounded" knowledge is clearly useful to students of political philosophy.

It may require a stretch of the imagination for many students of political philosophy to accept the claim that "the Platonic Socrates reveals his awareness of the basic pluralism of society and thus appeals to anyone who is not narrow himself in his conception of justice" (p. 170). Yet it is just such alternative views that lead to an intelligent understanding of the basically dialectical nature of political thought. In this sense Sullivan stirs interest in further examination of the "model of discourse rationality" developed in philosophical hermeneutics. Yet one would be better served to read Gadamer than Sullivan. Students of politics will not find here a clear, concise, and useful understanding of how hermeneutics can be useful in the contemporary study of politics. Someone more attuned to the discourse of contemporary political science needs to tackle this subject if a serious dialogue between philosophical hermeneutics and political science is envisioned. Every dialogue needs at least two participants with some fundamental understanding of a common problematic. Sullivan's work, for all its insights into twentieth-century German thought, does not provide a basis even to begin the discussion.

BRUCE E. WRIGHT

California State University, Fullerton

The Grammar of Modern Ideology. By Bernard Susser. New York: Routledge, 1988. 429p. $55.00.

Bernard Susser weaves a most interesting fabric of ideological interpretation in his endeavor to discern the grammar of modern ideology. He main thesis is that contemporary ideological discourse "shifts from the premodern generic sentence, 'I am right, you are wrong' to its modern variant, 'I am right, you are relative' " (p. 118). This claim and Susser's treatment of it clearly distinguish his work from such noted students of ideological structuring as Macridis, Hagopian, Hoover, Dolbeare, Medcalf, and Skidmore. While these scholars are principally concerned with the substance and issues of ideology, Susser focuses on the form and description of language, where "ideology appears as a series of prescribed, predictable, even compulsory utterances that conform to a system, rather than as an irreducible, substantive, response-demanding claim on human loyalty" (p. 90).

Susser's thesis rests on two ostensibly contradictory axioms: (1) value, meaning, and what is significant are conventional constructs with only relative validity; and (2) the world is fundamentally knowable. From these two conflicting presuppositions "the grammatical uniformity of ideological thought is generated" (p. 91), the "modernist dilemma"—epistemological crisis—emerges. To cast the modernist predicament in a somewhat different light, we can say that the principal dilemma of modern ideological thought is the insistence on establishing unshakable foundations of sociopolitical engineering on the shifting sands of "the essentially contestable status of moral judgments" (p. 57).

While there is an element of truth in the claim that all ideologies aspire to methodological purity and attempt to advance claims of value independence, Susser believes their relentless preoccupation with identifying the "ideal Archimedean point" stands at the center of the modernist dilemma. This Archimedean preoccupation involves a significant tension. On the one hand, rendering value judgments unnecessary is the primary function of the Archimedean position. On the other hand, a fundamental congruence of spirit, style, structure, and substance exists between the ideological structure and its Archimedean shelter (pp. 96-97). However, inherent within this fundamental congruence is the spirit of relativism itself. Susser argues, for example, that ideological protagonists such as Sartre, Germino, and Feuer dislodge their "adversary from the narrow island of epistemological certitude upon which [they] claim to stand, forcing [them], thereby, into the waiting sea of relativism—

and this without engaging in direct debate on first principles" (p. 42). As with the protagonists of ideology, so with ideologies themselves; relativism is a significant part of their interpretative processes.

In order to demonstrate the cogency of his thesis and to substantiate its claim of credibility, Susser turns to the grammatical form of ideological discourse. This form consists of 10 rules, which have been methodologically extracted from the varieties of ideological commitment. Having formulated these rules, Susser turns to their application. Beginning with the sociologists of knowledge (Scheler, Weber, Mannheim, and Lukacs) he argues that they, more than any other single intellectual group, yield "a generic grammar of the language that modern ideology speaks" (p. 57). These thinkers and the changes they exemplify have, according to Susser, "pervasively transformed modern ideological thought. Residual relativism, in both its relative and absolute moments, as well as the relativizing style in ideological debate, are written into virtually all the major social doctrines of the age, regardless of their degree of overt reflexiveness" (p. 94).

From the sociologists of knowledge Susser moves to an analysis of the "restorative ontology" of Leo Strauss, Michael Oakeshott and "the cult of the pedestrian," John Dewey and liberalism as scientific intelligence, Karl Popper and fallibilistic absolutism and the open society, Georges Sorel and the "will to deliverance," and Herbert Marcuse and "reason and the human telos." With the precision of an accomplished surgeon Susser spells out in detail both in what sense the rules of ideology apply to these major intellectual doctrines and how these intellectuals contribute to the grammar of modern ideology. According to Susser, the "challenge and fascination of grammatical analysis lies in the relationship between its *langue* and its *parole*, that is, in the virtually limitless capacity for creativity, the unexpected variations and ingenious turns one confronts in which arguments that are unquestionably sui generis in substance and in style prove to be, on closer inquiry, part of a law-governed and predictable form of discourse" (p. 131).

Susser is an accomplished writer, and *The Grammar of Modern Ideology* is worth reading. His work shows us a side of ideology that deserves continued and serious study. If the book is flawed, surely its flaw takes the form of reductionism; that is to say, with mindful deliberation one could well argue that all social doctrines are subject to the generic grammar of the language that modern ideology speaks. In short, every social paradigm is ideologically laden.

DAVID A. FREEMAN

Washburn University

The Immanent Utopia: From Marxism on the State to the State of Marxism. By Axel van den Berg. Princeton: Princeton University Press, 1988. 580p. $65.00 cloth, $16.95 paper.

This extraordinary volume aims not only to survey the entirety of Marxist literature on the state but also to derive therefrom a general assessment of the entirety of Marxism. In addition, this is an explicitly critical survey and assessment. Van den Berg defines himself as a non-Marxist examining the theoretical quality of Marxism (a task that poses no epistemological or philosophical problem worthy of his consideration). To the question whether it offers a preferable alternative to conventional non-Marxist theories of the state or even any coherent theory at all, he answers with a resounding *no*.

The author finds an ultimate cause for what he variously refers to as Marxism's "failure," "degeneration," and so on: it lies in what he calls Marxism's "immanentist core," namely, its unexamined presumption that the workers' social situation impels them necessarily—that is, as an immanent reality—to favor and seek socialism as against capitalism. Since it is clear to the author (the word *obvious* appears very often in this text) that workers have not actually favored socialism, Marxists have had an ever-more-difficult time justifying their projects or even thinking coherently about social development. They have been too busy excusing, via tortured and obscurantist theorizing, the divergence of reality from their unexamined presumptions.

To reach this strikingly unoriginal conclusion, there are several chapters of detailed and generally derisive critiques of (chiefly contemporary) Marxist theories of the state. These are argued to be more influenced by, and absorptive of, non-Marxist theories than has been recognized by both Marxists and non-Marxists. An interesting case is made that some Marxist theories mix contradictory logics without concern for, or even awareness of, the problems this poses for their conclusions.

However, the author's wearisome and dismissive contempt for Marxism makes this book of little interest or use. Large portions of contemporary Marxist theory, including discussions of the state, are ignored altogether. Major writers (Gramsci, Trotsky, Lukacs, Althusser, and Balibar, to name but a few) are brushed aside with such minimal and derisive attention as to cast serious doubt on the conclusions the author presents. Little new is offered here in the way of criticism of Marxism. Indeed, the author painstakingly and repeatedly cites the other critics of Marxism on whom he relies so heavily. Several of their critiques of Marxism remain significantly more subtle, more careful, and often far more balanced than this book.

Perhaps the most serious weakness of this book lies in the gap between its very worthwhile purpose—a non-Marxist critique of the state of Marxism—and its contents. To assess the state of Marxism—the author's explicit goal—requires first and foremost a grasp of its current and historically unprecedented period of change. Yet the author seems either not to know or not to care to contend with this "state" of Marxism. Starting in the 1960s, the accelerating erosion of "official Marxism" (the hegemony within the Marxist tradition of the USSR's approved version of Marxist theory) has provoked (1) the rediscovery of formerly repressed dissident Marxist theories, (2) a surge of critical engagements with important recent changes in non-Marxist theories (e.g., poststructuralism, postmodernism, feminism), and (3) the development of new kinds and new directions of Marxist theorizing. These changes have in turn generated all manner of new debates including discussions of the state. A remarkable "rethinking of Marxism" has been underway virtually on a global scale for at least the last 10–20 years.

No one can know where this will take Marxism, whether a new theory will achieve hegemony within the Marxist tradition (and if so which one) or whether that tradition will be the site of intense theoretical debate for years to come. In either case, no assessment of the state of Marxism or its relevance to the debates among non-Marxists should proceed as if this most profound transformation in its history were not occurring. Yet that is what this book does, thereby being profoundly out of date as it appears. In terms of the vast theoretical terrain it simply overlooks or dismisses and also in its relentlessly polemical tone this book is neither relevant nor attuned to the important new kinds of critical dialogue that increasingly engage Marxists and non-Marxists.

RICHARD D. WOLFF

University of Massachusetts, Amherst

Whose Keeper? Social Science and Moral Obligation. By Alan Wolfe. Berkeley: University of California Press, 1989. 371p. $25.00.

Alan Wolfe's book is impressively lucid, solid, and particular. His book is simultaneously a work of social theory and of moral and citizenly concern; and both the social theory and the moral concern are solidly grounded in empirical data. This book is both solid social science and empirically grounded moral and political theory. It is, you might say, an excellent example and confirmation of Kant's maxim, "Concepts without percepts are empty; percepts without concepts are blind."

Wolfe is centrally concerned with the question how and from what sources people in modern societies form and perform their social and moral obligations in both small- and large-scale settings. There is, in Wolfe's view, a deadly moral vacuum at the heart of modern societies. "Modern people need to care about the fates of strangers, yet do not even know how to treat their loved ones. Moral rules seem to evaporate the more they are needed" (p. 5). Tradition, religion, local community, literature, moral philosophy, and social science all fail as sources of moral guidance. Modern societies are on the brink of moral bankruptcy.

This, of course, has been often and eloquently said by many other students of modernity. Wolfe's particular and powerful contribution to the discussion is to show, in solid and thoughtful detail, how the practices of both the market and the welfare state (along with the theories that explain and justify those practices) work to reduce the vitality of what theorists since Hegel have taken to calling "civil society."

Wolfe looks at the United States to see how market institutions and theories have shaped work, family life, schooling, place and neighborhood, and intergenerational connection. He examines in angry but controlled passion how the economic science of the Chicago school would try to make economic man the whole man and how the political science of the public choice and rational schools supplant moral and humane thinking with technical or authoritarian calculation. He examines in rich detail and with a keen moral eye how both modern market economies and the modes of reasoning associated with them prey on society, consuming the networks of kinship, custom, and affiliation that once gave people guidance and meaning. Wolfe then turns to Scandinavia, home of the benevolent social welfare state, and examines how, while the theory of the social welfare state at least acknowledges that we have obligations to one another, its actual practices weaken the social and moral ties of family and community. As the state expands, society contracts. What began as state action to transfer money from those who had more of it than they needed to those who had less of it than they needed now increasingly takes the form of the state's building institutions and providing services that families and other social institutions once provided. Wolfe reports, for example, that in Denmark the number of children in state-supervised foster homes increased by 400 percent between 1970 and 1980 and that in 1985 only 5 percent of children under age six were cared for full-time by a parent or parents at home. More and more young people spend more and more of their time in state institutions and programs. The "public family" grows while the private family declines. Families and communities are increasingly organized by state rules rather than by internally generated moral understandings. Meanwhile, Wolfe reports, there is throughout Scandinavia rising concern over the moral consequences of the welfare state as alcoholism, crime, and tax evasion all seem to be on the rise, while attitudes of personal and familial responsibility and interclass and intergenerational affiliation seem to be on the decline.

Alan Wolfe wants to redeem and replenish the "gift of society" from the assaults of both market and state. He has written a work of solid social science and the best kind of empirical political theory. I think that his book, perhaps along with Michael Ignatieff's *Needs of Strangers,* exemplifies the best kind of thinking about community.

JOHN H. SCHAAR

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AMERICAN POLITICS

The Ambition and the Power: The Fall of Jim Wright: A True Story of Washington. By John M. Barry. New York: Viking Penguin, 1989. 768p. $22.95.

On 31 May 1989, Representative James C. Wright, Jr. announced his intention to resign from the U.S. House of Representatives. For more than thirty four years he had represented the 12th District of Texas, the last three years as Speaker of the House. In *The Ambition and the Power,* John M. Barry, an experienced and unusually resourceful journalist, chronicles Wright's rise to power, his initial successes as leader of the Democratic party in the House during the 100th Congress (1987–88), and the events that led to Wright's downfall as the result of forceful, even abrasive, leadership and, later, alleged ethical violations of the House rules.

In the 202-year history of the House of Representatives, three remarkable Texans have served as Speakers: John N. Garner (1931–33), Sam Rayburn (1940–46, 1949–52, and 1955–61), and Jim Wright (1987–89). The stormy partisan Garner went on to serve as vice-president under Franklin D. Roosevelt for eight years; the highly respected and beloved

Rayburn compiled a long and distinguished record as Speaker—17 years. And the ambitious, hard-driving Wright, with Rayburn as his model and mentor and ably assisted by majority leader Tom Foley of Washington and majority whip Tony Coelho of California, established one of the most successful legislative records that the House Democratic party had been able to achieve in decades.

Barry provides a fascinating, insider's perspective on the legislative process, the strengths and foibles of the principal actors—not just Wright, Foley, and Coelho and their staff support but also Republican leaders Bob Michel of Illinois, Dick Cheney of Wyoming, and Newt Gingrich of Georgia. It is the militantly conservative and ambitious Gingrich who serves as the principal opponent to Wright, stirring up the media fire storm that ultimately led to Wright's investigation before the bipartisan House Committee on Standards of Official Conduct (Ethics Committee).

What makes this book unusually valuable to students of congressional leadership is the remarkable access that Barry obtained from Wright, over the objections of most of his staff. Barry parlayed this access into contacts with other members of Congress, basing his book on over two hundred interviews with some 60 members, as well as countless conversations with staff, lobbyists, and reporters. Barry, a former Washington editor for *Dun's Business Month* and freelance writer, writes with skill and insight about how the game of politics is played on Capitol Hill, the pressures on congressional leaders to succeed, and how the best-laid plans of lawmakers can fall apart.

Sometimes the sports metaphors are overdone; headnotes from Machiavelli's *The Prince* (1513) and quotations from Canetti's *Crowds and Power* (1960) come across somewhat heavy-handedly; but the intensity of Wright's saga, the cavalierism of Wright's Texas-style business practices, and the Ethics Committee response are high drama, indeed, and convincingly told.

What led to Wright's downfall? Barry, consistent with his themes of ambition and power, lays a great deal of the blame on Wright's personality, an unusual mixture of hubris and single-mindedness that often found Wright going it alone, as in his granting Barry nearly unlimited access to leadership meetings. (Where top staff could go, so too could Barry.) Wright's arbitrary use of the rules, including a long count on a critical budget reconciliation bill, allowed his aides to round up a *no* vote from a fellow Texan and convert a loss into a stunning 206-205 Democratic victory. Wright's actions greatly alienated the Republican minority. In February 1988 Wright's inability to forestall a vote that would have allowed members a 51-percent pay raise, lost him the support of many of his Democratic colleagues. In May, during the final stages of the investigations of his congressional ethics, spurred on by Gingrich, Common Cause, and the media, Wright's cause suffered a further psychological setback from the controversial resignation of a trusted floor lieutenant, John Mack. Wright, confronted with three major ethical charges—all of which Barry attempts to refute, not always convincingly—decided to step down. He was well aware that no matter what the Ethics Committee investigation outcome, his ability to lead as a Speaker would be irreparably damaged.

The Ambition and the Power is an excellent contribution to what is known about congressional leadership and the workings of the legislative process. It is the best book written on inside-the-Beltway politics since Hedrick Smith's equally admirable study, *The Power Game: How Washington Works* (1988). Both books depend on sustained access of the kind that political scientists, for the most part, can only envy. Barry's book, as strong as it is, would have been improved with editing to remove much repetition, some notes, and an index. One can only hope that the author and publisher will consider bringing out an abbreviated version (400–450 pages) for possible classroom adoption. This exceptional book deserves a broader, collegiate audience.

ROBERT L. PEABODY

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Strategic Intelligence for American National Security. By Bruce D. Berkowitz and Allan E. Goodman. Princeton: Princeton University Press, 1989. 232p. $19.95.

Most works on U.S. intelligence are either highly specialized tomes that address one facet of the intelligence process, in-depth case studies of U.S. intelligence at work (or its maladroit application), or issue-oriented polemics. This book takes none of these usual forms. Instead, Berkowitz and Goodman give us a very systematic, thorough, authoritative survey that applies solid political science, policy science, and political theory to the important study of the gathering, processing, and utilizing of U.S. intelligence. Based on the experiences of both authors plus extensive interviewing, they offer a book of rich insights for academic and practitioner alike.

At the outset, the authors define *strategic* intelligence as very different altogether from *operational* or *tactical* intelligence, strategic intelligence having "a wider base and broader objectives, integrating economics, politics, social studies, and the study of technology" (p. 4). Its purpose is to provide decisionmakers with the big picture and long-range forecasts. The authors cover strategic intelligence very comprehensively with chapters on the contemporary intelligence environment; intelligence collection; intelligence analysis; organizational problems; the immediate issues of funding, personnel, and organization; and the challenge ahead. Additionally, useful appendixes are provided: a checklist of various intelligence pathologies, malfunctions, and other things that can go wrong (along with the likely causes) and a useful glossary of common terms and acronyms.

Their work is good political science in that it is methodologically sophisticated and additive. Building on Sherman Kant's 1949 book, *Strategic Intelligence*, the authors attempt to illustrate how intelligence is both similar to, and different from, processes and issues addressed by Kant. Their methodological sophistication is most evident in their (1) framework for studying intelligence (consumer needs, information collection, analysis, organization, dissemination); (2) descriptions and analysis based on extensive research; (3) excellent use of taxonomy for classifying the different sources of collection (human, signals, and imagery); (4) "loose factor theory" of the various sources of information (known facts, secrets, assumptions, best estimates, uncertainty, disinformation, and mysteries); (5) treatment of intelligence and type 1 and type 2 errors (false positives and false negatives, respectively); and (6) excellent case study into the source of estimate errors in the 1950s and 1960s concerning Soviet ICBM's.

This book is good policy science in three areas. First, Berkowitz and Goodman nicely sketch the changing intelligence (policy) environment: the multiplication of targets (object of studies), the multiplication of consumers, the increased difficulties of collecting data from modern technology and totalitarian government, and a new political context of congressional oversight and adjudication based on the Freedom of Information Act. In their words, "the intelligence environment of today is considerably different from that which existed when the intelligence community was first founded, and the future success of the community will depend greatly on how well it deals with this new environment" (p. 28). These new constraints make for a different ball game.

A second example of solid policy study is the strong emphasis throughout on the disparity between structure and practice: "The intelligence cycle is one thing in principle and quite another in practice. Most bureaucracies rarely operate precisely according to flow charts, theoretical models, and organizational tables" (p. 30).

A third example is found in their use of the bureaucratic politics and organizational pluralism in accounting for contradictory and competing assessments. Systemic fragmentation is emphasized as a major causal explanation of U.S. intelligence process and politics.

Finally, this book is good political theory in that it raises some of the profound questions posed by intelligence activities in a free society and makes recommendations concerning them. Although they may devote too much attention to the more mundane issues of budgets, personnel, and organization and not enough to control, accountability, and responsiveness, they are persuasive when writing that "if a democratic government is to have an effective intelligence service, these issues must be discussed" (p. xii).

A number of normative prescriptions for better practicing the "craft of intelligence" and for balancing democracy and security (pay attention to all phases, play effective bureaucratic politics, keep the consumer in mind, coordinate, and avoid surprises) seem right on target.

This book offers much. It sells the virtues of realism, balance, methodology, and coordination. The authors' conclusion about the importance of playing by the rules of a pluralistic state merits reflection: "The first strength [of the U.S. intelligence community] . . . is the acceptance today by U.S. officials and the American public" (p. 169). Effective intelligence requires that this support be neither strained nor squandered.

DAVID C. KOZAK

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Politics and Society in the South. By Earl Black and Merle Black. Cambridge: Harvard University Press, 1987. 363p. $25.00 cloth, $10.00 paper.

The South's New Politics: Realignment and Dealignment. Edited by Robert H. Swansbrough and David M. Brodsky. Columbia: University of South Carolina Press, 1988. 330p. $34.95 cloth.

Arkansas Politics and Government: Do the People Rule? By Diane D. Blair. Lincoln, NE: University of Nebraska Press, 1988. 362p. $25.95 cloth, $15.95 paper.

For almost forty years, political scientists have produced a vast literature on Southern politics, while awaiting the second coming of V. O. Key. The longing for another masterpiece such as *Southern Politics in State and Nation* (1949), will probably never be fulfilled: the nationalization of Southern politics, the region's internal diversity, and, above all, the abundance of evidence and interpreters make a new synthesis unlikely. Still, the South's pivotal role in U.S. politics requires us to keep looking.

Meanwhile, the three volumes reviewed here provide a good substitute. All make vital contributions to our understanding of Southern politics, especially in the electoral arena. All assess fundamental social transformations in the South and trace their influence on partisan competition. All address Key's well-known questions on the implications of politics for the distribution of power and wealth. And, read together, they dispel a few clouds of ambiguity obscuring the future of Southern politics.

The most ambitious volume is Earl and Merle Black's long-awaited *Politics and Society in the South,* which approaches Key's mastery in its historical sophistication, literary grace, and statistical elegance. (Indeed, the maps, tables, and charts alone are worth the price of the book.) Put all too briefly, the Blacks show how economic modernization, urbanization, in- and out-migration have created a South quite unlike Key's agrarian oligarchy, run by black-belt segregationist Democrats. The rise of the white middle class, the enfranchisement of blacks, and the political mobilization of both blacks and whites have led (roughly as Key predicted) to competitive two-party politics.

But contrary to Key's dream of biracial liberal majorities, competition ushered in a new conservatism. The racial and social attitudes of the new middle class, buttressed by an entrepreneurial version of individualistic political culture, fed Republican growth. After 1965 the GOP became dominant in presidential politics, competitive in statewide races, and at least challenging on the local level. At times the authors see Republican hegemony as inevitable: the reality for liberal Democrats is that "there are simply not enough progressive whites to unite with blacks in most Southern states to provide regular electoral majorities" (p. 230). The best to be expected are some victories by Democratic conservatives (a.k.a. "moderates"). In any case, conservative state public policy will "perpetuate much of the past" (p. 316).

The picture looks muddier—but more hopeful for Democrats—when the analysis drops to the state level. Swansbrough and Brodksy's *The South's New Politics* provides strong evidence of electoral diversity and Democratic viability. Although the book is marred by repetitive, mechanical, and sometimes inconsistent discussions of party system change (no one seems to have read Stanley and Castle's fine chapter on realignment), the authors of the state chapters use both election returns and poll data to paint a fascinating picture of how economic and social modernization, described so well by the Blacks, has produced varied partisan results. These range from continuing strong party ties and Democratic supremacy in Kentucky (fostered by party registration and closed primaries) through dealignment in South Carolina to growing GOP identification in Florida. And although the state polling data are less satisfactory on this point, the factors underlying party alignments also seem to differ. Race divides Republicans and Democrats everywhere; economic status undergirds party divisions in all states, but not to the same degree; issues dominate in some (North Carolina), but not in others (Alabama); and short-term forces such as party and candidate images (especially Reagan's) are important everywhere.

That economic and demographic changes are not always conducive to Republican majorities and conservative politics is further demonstrated by Blair's fine case study, *Arkansas Politics and Government.* After a fascinating review of Arkansas's history of pervasive impoverishment, political traditionalism, and elite malfeasance, Blair pursues the impact of modernization through almost every sphere of government, from the governor's office to the court system. On balance, she concludes, "Better politics can produce, and occasionally has produced, better public policy" (p. 263). Crucial to "better policy" are continuing Democratic victories, bucking trends that ought to bolster the GOP. Why have the Democrats prospered? Because they have run attractive, personable candidates like Dale Bumpers, David Pryor, and Bill Clinton, who skillfully massage the progressive side of Arkansans, appealing to liberal or populist instincts long "suppressed or diverted in the traditional political system" (p. 272). These Democratic entrepreneurs, attuned to local political culture, have campaigned on widely shared concerns, such as better education and government services, evoking the "progressive" biracial majority sought by Key.

Although the Blacks might interpret such victories as holding actions in the face of GOP advances, recent developments suggest otherwise. Strong Democratic showings in the 1986 and 1988 congressional elections and recent gubernatorial victories, such as that of Douglas Wilder in Virginia, hint that similar strategies may be quite viable for many Democratic candidates and (if Swansbrough and Brodsky are correct) not just in Arkansas. Whether Democratic presidential candidates can utilize this approach to win the South is more problematic.

Taken together, then, these volumes provide a panoramic view of the politics and possibilities of the modern South. Nevertheless, in the inevitable comparison with *Southern Politics,* these works neglect some topics that were—or would be—of interest to Key. Despite frequent mention of elites and elite strategies, not much attention is paid to party leaders and activists (excepting one chapter in Swansbrough and Brodsky). In the same vein, little is said on another of Key's favorite topics—political money, one of the South's major crops. And the mobilization of conservative Christians in the 1980s, especially within the GOP, barely surfaces in three books on the politics of the Bible Belt. But these complaints are premature: more will be written on these and other issues, much of it by the authors represented here.

JAMES L. GUTH

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In Vitro Fertilization: Building Policy from Laboratories to Legislatures. By Andrea L. Bonnicksen. New York: Columbia University Press, 1989. 194p. $28.00.

We are now in the midst of a revolution in human reproduction. A wide array of genetic and reproductive technologies raises critical policy issues concerning the roles of government in an area that until recently was assumed to be a private matter. The growing involvement of third parties and increased dependence on expensive technological solutions to infertility, however, are rapidly changing the context of reproduction and challenging conventional notions of procreative freedom. In this setting it is crucial that political scientists be major participants in the dialogue over social priorities and government responsibility. To date, such involvement has been sadly lacking.

Andrea Bonnicksen's book on in vitro fertilization (IVF) is a good example of the kind of scholarship that needs to be done by more members of our discipline. This thorough, well-researched, and highly readable book is marked by its balanced treatment of this politically charged topic. Although the book focuses on one technique, the author's insights on the policy dimensions of IVF represent a valuable perspective on policymaking in general.

Bonnicksen contends that there are two worlds of IVF: the public's legal, political, and ethical concerns about reproductive technology and the personal, pragmatic world of patients seeking a cure to infertility. She argues that we must separate the two and analyze IVF on its own terms. Furthermore, each of the communities involved in IVF (physicians, patients, government, and public) must assume responsibility for circumspect consumption of it. Bonnicksen argues that elaborate legislation is not now appropriate; formal public policy should be delayed until we gain better understanding of IVF values and practices. According to the author, "we need to move slowly, cautiously, and with confidence as we shed self-defeating ideas of slippery slopes and technological imperatives and turn to the human and manageable dimensions of IVF" (p. 138).

In the early chapters the author gives a good review of the history and clinical use of IVF and an in-depth analysis, based on firsthand observation and interviews, of its impact on patients. Chapter 5 on responsible government is of most direct interest to political scientists. In it Bonnicksen analyzes private and public efforts to monitor IVF and demonstrates how this technique got caught up in abortion politics and fears of genetic engineering. Misguided policy, she argues has put the cart (clinical applications) before the horse (basic research on IVF) and further clouded the issues (p. 82).

Bonnicksen clearly favors an incremental approach: "Policy ought to be built gradually and carefully from voluntary regulation, state by state modeling, and, if needed, federal legislation" (p. 113). Her preferred course of action would include voluntary standards in the private sector, periodic monitoring by the states, codification of standards by the states when voluntary compliance is found ineffective, and uniform state laws. The only roles that she appears to trust the federal government with at this time are affirmative in nature: creating a national commission to serve as a forum for a dialogue over reproductive technologies and confer legitimacy and resuming funding of IVF research after a ten-year de facto moratorium.

Based on the government response to IVF over the last decade, Bonnicksen is understandably wary of regulation. Although she agrees that there might be some extreme (but unlikely) extensions of IVF research that need to be banned, even regulations are now premature because in the present political climate IVF remains "vulnerable as a sacrificial issue that absorbs the ire legislators feel toward abortion policies" (p. 98).

Because of the author's preference to insulate clinical IVF from the broader issues surrounding reproduction, however, there is only limited discussion in this book about the nature of the opposition. One of the most interesting aspects of the politics of new reproductive technologies is the extent to which opposition is spread across the political spectrum. Although Bonnicksen is correct that the most salient condemnation has come from right-to-life interests, significant concern has been articulated by traditionally liberal interests including civil libertarians, minority group leaders, rights advocates for handicapped persons, and feminists. The fact that this opposition exists across traditional lines of cleavage helps explain more fully why elected officials are so hesitant to enter the fray. My feeling is that if concern over IVF was limited to the political Right, the policy context would be far different. A more in-depth analysis of the interest group dynamics would help to clarify the political milieu of IVF.

Although I believe Bonnicksen is right in arguing that IVF as practiced must be separated from the worst-case scenarios that serve as ammunition for opponents, her focus on clinical applications underestimates the revolutionary—and thus controversial—character of IVF. With the capacity to move conception and early embryo development from the secrecy of the womb to the lights of the laboratory comes the possibilities of genetic intervention in a *Brave New World* sense. It is not simply that IVF has wrongly become a symbol of genetic engineering; in fact, external fertilization is a pivotal and necessary step for more intrusive manipulations of the human embryo. To some extent the centrality of IVF to all future extensions answers Bonnicksen's question, "Why do we both deify and satanize a technique that unites microscopic eggs and sperm in a Pyrex petri disk?" (p. 5). While some observers might deify the clinical attempts to bypass infertility in individual patients, others are justifiably concerned about where external fertilization might ultimately lead us.

Robert H. Blank

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The Great Challenge: The Myth of Laissez-faire in the Early Republic. By Frank Bourgin. New York: George Braziller, 1989. 223p. $22.50.

The Decline of Authority: Public Economic Policy and Political Development in New York State, 1800–1860. By L. Ray Gunn. Ithaca: Cornell University Press, 1988. 284p. $29.95.

The books under review offer contrasting perspectives on U.S. political economy in the formative years of the republic. Frank Bourgin's *Great Challenge* seeks to uncover antecedents to New Deal-style state intervention in the economy and to explode what he calls the "myth of laissez-faire." Bourgin focuses on the intentions of the framers, emphasizing their commitment to internal improvements and economic development. Ironically, as chapters on Alexander Hamilton's ambitious designs demonstrate, the framers' intentions were largely thwarted: if laissez-faire is not the ground principle of the new regime, it seems—from the federal perspective—a pretty accurate description of historical reality. The reality was, of course, much different in the states. "Historians have long since laid to rest the myth of laissez-faire," writes L. Ray Gunn in his important new study of political development in antebellum New York (p. 99). The challenge to modern scholarship is not to recover a lost tradition in political economic thought but rather to explain the withdrawal of government from an active role in the economy after decades of successful state intervention.

Bourgin's book is a publishing curiosity. An earlier draft was rejected by his Ph.D. committee at the University of Chicago in 1945. Revived and rewritten forty years later and boosted by a strong endorsement by Arthur Schlesinger, Jr., *The Great Challenge* earned Bourgin a belated doctorate and now publication by a prominent trade publisher. It would be gratifying to report that the book makes a significant contribution to scholarship. Unfortunately, this is not the case. Bourgin's "myth" has long since been exploded and scholars have moved on to more interesting questions.

Many of these questions are provocatively explored in *The Decline of Authority*. Gunn's major concern is with the increasingly problematic relationship between politics and economics in antebellum New York. From 1800 to 1840 state involvement in the economy spurred massive infrastructural development and economic growth. This was the great era of "distributive politics," when despite—or perhaps rather because of—a "very poorly developed sense of political community" and a highly decentralized political system, government successfully met a wide array of constituency demands (pp. 97–98). The state legislature functioned as a kind of "public market," in which representatives of "local and special interests bargained and traded" for advantages (p. 80). High turnover meant that "the boundaries between the legislature and society were fluid and indistinct," thus guaranteeing that the assembly would lack the institutional capacity to formulate coherent policy (p. 75). New York's political system functioned effectively as long as outputs could be widely distributed in an expanding political economy. By 1840, however, severe financial dislocations resulting from the panic of 1837 demonstrated the limitations of distributive politics: "existing political structures" could no longer satisfy "multiplied political demands"; "The result was a crisis of legitimacy" (p. 8).

Gunn is at his best in delineating systemic responses to this crisis. New Yorkers saw "further democratization" as the answer to legislative excesses (p. 169). Harking back to the radical revolutionary tradition, the new Constitution of 1846 curbed the legislature, extended popular participation, and devolved new powers on local government. At the same time, however, the crisis of the 1840s precipitated a general "withdrawal of the state from economic activities," thus accelerating the long-term privatization of the economy (p. 166). "The price of maintaining the legitimacy of the political system," Gunn concludes, was "emasculation of government" (p. 197). Yet even as government's policymaking capacity diminished, the system as a whole became more recognizably "modern": "administration and adjudication began to replace the traditional politics of distribution with more rational, stable, and predictable mechanisms of allocation and legitimation" (p. 221). These developments produced a "new configuration of politics" and assured that "the American version of democracy would be exclusively political and procedural, and that much of social and economic life would be outside the boundaries of politics" (p. 16).

Gunn's explanation for the transformation of New York's political system is ingenious and persuasive. Only occasionally do his theoretical commitments lead him to strike an anachronistic note, particularly when he berates New Yorkers for failing to develop and implement a coherent conception of the public good. From our contemporary perspective, the failure of government to manage and direct the great "release of energy" that government interventions in the economy had made possible in the first place may appear to be a great lost opportunity with lamentable consequences for the future course of U.S. development. But New York's successive political systems worked for, and seemed legitimate to, most New Yorkers. Legitimacy, this excellent study shows, is not a function of systemic rationality or of an ideal fit between political and economic spheres. *The Decline of Authority* provides a superb case study of the complex and contradictory ways in which political systems evolve toward, and shape the character of, modernity.

PETER S. ONUF

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Migration and Politics: The Impact of Population Mobility on American Voting Behavior. By Thad A. Brown. Chapel Hill: University of North Carolina Press, 1988. 198p. $29.95.

Three decades ago Angus Campbell and his colleagues used the University of Michigan Survey Research Center surveys to analyze the effects of regional migration on the U.S. voter. For most citizens, these effects were negligible. Among the four main categories of migrants, only Southern blacks moving to the North appeared to change their political attitudes. These results were consistent with the theory that early socialization tends to immunize citizens against political change. That blacks who left the South changed their political attitudes could be accounted for fairly easily. As Southern blacks had little opportunity to participate in politics in their native region, they were not immunized against change.

Two decades later Thad A. Brown challenged this conventional wisdom ("On Contextual Change and Partisan Attributes," *British Journal of Political Science* 11 [1981]). Rather than merely look at movement from region to region, Brown classified migration according to movements to and from countries and congressional districts with differing levels of Democratic voting. Using the 1970 National Election Study (NES) survey, Brown found that such movements did lead to changes in congressional voting, with respondents often shifting their vote to the predominant party of their new district. Migrations were less likely to lead to changes in party identification.

Brown's article provided a preview of the more extensive findings reported in his *Migration and Politics.* Brown skillfully discusses alternative theories about the effects of migration, raising the possibility that an openness-to-change model might better explain the behavior of migrants than a model that views migrants as immunized against change.

To test hypotheses about the effects of migration, Brown conducts a secondary analysis of three major studies: the 1970 NES midterm election survey, the 1980 NES presidential election survey, and the panel study of U.S. high school seniors and their parents conducted by the University of Michigan Survey Research Center in 1965 and 1973. Although Brown studies the effects of migration from region to region on individual-level political attitudes and behaviors, his most impressive analyses rely on his own measure of the effects of local political environments on political change. Brown builds on and improves his earlier measure of local political environments. As with his earlier work, he relies upon county-level and congress-level voting statistics; but he increases the number of elections used to measure the past and current local political environments of each migrant. Brown classifies respondents according to whether they moved to "congruent," "mixed," or "incongruent" political environments. He finds that more than half of the migrants who move to "incongruent" environments change their vote to match their new political environment, although many resist change. However, they do not usually switch their partisan allegiances. Rather, they partially adapt to their new political environment by becoming political independents. Brown concludes that a model of political learning that predicts change throughout the life-cycle better accounts for the impact of migration than a model based upon a theory of political "immunization."

I cannot here do justice to the richness of Brown's findings. These findings are presented with remarkable clarity. The results are presented in tables and figures that are easy to interpret; and six of the eight chapters (all but the introduction and the conclusion) end with clear summaries of Brown's main points and his main research findings. Two added virtues: the book is short and well written.

Although I would strongly recommend Brown's book to scholars of U.S. voting behavior—and it will be useful to scholars of comparative political behavior as well—further analysis is necessary. First, the most recent survey analyzed by Brown was conducted in 1980. In 1984, there was a substantial shift toward Republican party loyalties among the electorate, raising the possibility that more recent surveys might yield different results and raising the question of whether migrants contributed disproportionately toward this pro-Republican shift. Second, Brown's panel analyses rely on the first two waves of the Michigan student-parent survey, so young migrants are in their midtwenties. But a third wave of this panel was conducted in 1982, and this wave should provide better evidence on the long-term effects of migration. Brown cannot be faulted for failing to conduct a three-wave analysis, since the data for the three-wave study are not (as of 1989) available through the Inter-university Consortium for Political and Social Research. But one can hope that Brown or other scholars eventually will use the 1982 panel to study the effects of residential mobility.

Lastly, Brown's analysis of the macro-level effects of migration are not as well developed as his analysis of the effects of migration on individuals. For example, students of Southern voting behavior have debated the net impact of Northern migration to the South on the growth of the GOP.; but Brown does not address this controversy. Brown does argue that migration has the net effect of weakening partisan attachments and that migration "unravels the party system" (p. 155). But this argument might have been strengthened if he had provided estimates of the net effect of the contribution of migration to the growth of partisan independence or to the weakening of the New Deal coalition.

Despite these qualifications, *Migration and Politics* is an important contribution to the study of voting behavior and an outstanding application of contextual analysis. My criticisms all support that further research would be desirable. Brown's book gives us every reason to think that further research along the lines he advances would be rewarding.

PAUL R. ABRAMSON

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How Presidents Test Reality: Decisions on Vietnam, 1954 and 1965. By John P. Burke and Fred I. Greenstein. New York: Russell Sage Foundation, 1989. 339p. $29.95.

In the best case studies, analysis is motivated by theory, not subject matter. So it is in this book. The authors use a rich array of materials to provide insightful narratives of five presidential decisions concerning Vietnam; but the book's distinctive character rests on its explicit analytic framework for assessing how presidents test reality, defined as "the way presidents and other actors assess their environment" (p. 3).

The book's analytic framework is structured around three factors that influence whether "policy alternatives were systematically and rigorously addressed" (p. 3): the *president's advisory system*, especially its structure, dynamics, and products; the *president*, especially his core personal attributes and his dispositions for working with others and for responding to his political environment; and the *political environment*, especially the latitude it gives the president for making choices. The case studies consist of two Eisenhower decisions (whether to intervene unilaterally during the siege of Dien Bien Phu and how to generate a multilateral intervention option) and three Johnson decisions (whether to give U.S. military advisors more operational responsibilities, whether to escalate the U.S. military response, and whether to make an open-ended commitment of U.S. troops to South Vietnam).

The book has many strengths. Its organizational strategy greatly simplifies a potential morass of intermingled description and analysis. The first chapter presents the book's analytical framework. The heart of the book rests in the next 10 chapters, which are paired into narratives and analyses of the five case studies. This enables the authors to depict events coherently and then examine them with the analytic framework. The last two chapters compare Eisenhower and Johnson's reality-testing procedures and present eight summary propositions. The authors should also be commended for the book's collaborative nature. By integrating their efforts, they created a whole greater than the sum of their parts. Their teamwork offers a role model worthy of emulation. A final kudo for the authors' use of archival records. As post-FDR presidencies move into history's "middle distance," vast amounts of historical records become available for study. This book amply illustrates how they can shed new light on past events.

My concerns about the book are based on two ambiguities about the meaning of reality testing. First, it is unclear how much of the decisionmaking process falls within its bounds. Chapter 1 defines reality testing as the predecisional activities of "assess[ing] the environment" (p. 3). Yet the authors devote much of their effort to explaining the subsequent policy choices made by the presidents. This confuses the flow of the analysis, distracting attention from the process by which presidents generate and evaluate policy options. For example, virtually all discussion of the effects of the environment focus on how it affects presidential choices, not the reality-testing process. Ironically, those discussions of presidential choices have little bearing on the conclusions offered in chapter 12. Therefore, the authors could easily eliminate those diversions, yielding a more concise and focused treatment.

Second, the meaning of the term *reality* is unclear. Does it refer to testing the objective reality that exists independent of the president's preconceptions, or is it limited to testing reality as defined by the subjective schema that exists in the president's mind? Both presidents possessed schemas that limited their options: Johnson ruled out reductions or withdrawal of U.S. forces, and Eisenhower explicitly ruled out commitment of U.S. ground troops and implicitly ruled out consideration of whether the United States should have any commitment at all. Failing to recognize this distinction, the authors lump the two kinds of reality testing together but do not uniformly apply the same criteria to both presidents. They judge Eisenhower's reality testing process a success because it gave him a thorough evaluation of options that fell within his schema. However, his process appears to have fallen short of testing the objective reality, since it accepted (with one exception) Eisenhower's preemptory dismissal of the option of committing U.S. ground troops as "beyond contemplation" (p. 32) and failed to consider the option of an air strike at Dien Bien Phu (p. 60). Moreover, it never explored the possibility of making no commitment to Vietnam even though the authors feel that that may have been "*the* fundamental question" (p. 60). In contrast, the authors fault Johnson's process on both criteria: it failed to staff out options within the president's schema adequately, and it failed to challenge the schema itself. This ambiguity does not fundamentally compromise the conclusions of the authors, but making the distinction would have sharpened their analysis and clarified their grounds for comparing and evaluating the two presidents' reality-testing procedures.

These concerns notwithstanding, this book makes an important contribution to the literature on presidential decisionmaking, particularly by highlighting the values of formal structure and concommitant risks of informality. Most importantly, their propositions will I hope serve as a starting point for others seeking to test and evaluate presidential advisory systems.

CARY R. COVINGTON

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Acceptable Risk? Making Decisions in a Toxic Environment. By Lee Clarke. Berkeley: University of California Press, 1989. 229p. $25.00.

Lee Clarke writes, "The premise of this book is that organizations are central actors in assessing, mitigating, and accepting risks" (p. 4). He argues that "organizations, not disparate members of the general population, are the final arbiters of hazard" (p. 178). Clarke examines one case to make his argument that organizational processes are the critical elements in understanding why some risks instead of others become viewed as acceptable.

In 1981 a fire in a basement mechanical room contaminated all eighteen stories of the State Office Building in Binghamton, New York with PCBs and dioxins. Hundreds of people, mostly janitors assigned to clean up the building immediately after the accident, were exposed to the contaminants. In 1989 the building still remained closed. This book traces the organizational response to the accident through which responsibility for cleaning up the building was established and a medical surveillance program developed.

The main weakness of the book is that the story has little to do with acceptable risk. Clarke's data from interviews and documents show that risk assessments and concerns rarely drove organizational decisions. Instead, organizations sought to protect their interests, usually by limiting their involvement in the Binghamton situation. Even the organizations with the most significant roles in the cleanup did not view defining acceptable risks as a goal. Instead, definitions of acceptable risk were avoided, dealt with implicitly, or used to support decisions made for other reasons.

If the author wanted to focus on acceptable risk, he should have chosen a different case to study. As he points out, risk assessments had a small role in the Binghamton case, which involved an emergency followed by a lengthy cleanup. The case is analogous to the typical superfund toxic waste cleanup situation: everyone reaches agreement that the site is dangerous but under control for the moment. Risk assessments have a narrow role limited to questions of appropriate treatment. In other types of environmental cases questions of acceptable risk are often critical. For example, in situations involving siting of potentially risky facilities (e.g., nuclear power plants, toxic waste treatment facilities) questions of acceptable risk generally have a central role. In these cases risk assessments are important as governments ask local citizens to allow scary substances to come into their communities. There is a debate that often revolves around questions of risk. This sort of case would have provided useful information on the role of risk perceptions, communications, and assessments. In the Binghamton case debates about risks were, at best, muted.

Clarke criticizes the psychological studies of risk for their focus on individual perceptions and for lacking "a conception of social structure that connects perceptions of risk with the formation of policy" (p. 178). This seems gratuitous because the authors of the psychological studies do not claim to explain public policy formation but simply how individuals assess risks. They are not likely to object when other social scientists—perhaps political scientists trained in organization theory—use the findings from psychological studies to help develop such connections. Clarke is right that scholars have not paid adequate attention to the politics of risk assessments and the role of risk perceptions in policy formation. The problem, however, is not with the psychologists but with political scientists' failure to address these issues.

Although *Acceptable Risk?* tells us little about risk assessments, it is a fine study of the relationships among organizations faced with an unexpected local environmental crisis. Clarke applies a "garbage can" theory of interorganizational behavior to explain the constricting of the number of organizations involved in the case and for the eventual assignments of responsibility. Particularly impressive is a brief chapter that convincingly accounts for the absence of a strong local organization of concerned citizens.

Clarke does not demonstrate that "organizations . . . are the final arbiters of hazard" (p. 178). Instead, he has shown that organizations are the final arbiters of societal responses to a hazardous situation. This is not an original finding. Nevertheless, his book is valuable in showing how organizations react to an environmental hazard that disrupts and challenges their routines. The book makes a solid contribution to building a theory of organizational responses to drastic environmental hazards.

ROBERT E. O'CONNOR

Pennsylvania State University

Justice Rehnquist and the Constitution. By Sue Davis. Princeton: Princeton University Press, 1989. 247p. $19.95.

Justice Rehnquist and the Constitution is Sue Davis's timely examination of William H. Rehnquist's record as an associate justice (1972–86). During those years he earned a reputation as a lone ranger for writing more solo dissents than any other justice and for staking out the sharpest and most consistently conservative judicial philosophy on the Burger Court (1969-86)—which is why President Ronald Reagan elevated Rehnquist to the chief justiceship in 1986.

This is not a judicial biography or study of the influences on, and development of, Rehnquist's thought, however. It is a study of Rehnquist's judicial record and philosophy. In this regard it is more revealing than Rehnquist's own 1987 book, *The Supreme Court: How It Was, How It Is,* which disappointingly contains only guarded reflections on his service as a law clerk and a justice, along with a superficial overview of the Court's history.

In Rehnquist's judicial constellation, according to Davis, federalism (or "state autonomy") reigns supreme, with values such as private property and individual rights receiving less judicial protection. This is so, argues Davis, because Rehnquist is a thoroughgoing legal positivist, who ostensibly believes in a strict separation of law and morality and that judges should not "make law." Legal positivism undergirds Rehnquist's deference to the operation of majoritarian democracy. In addition, Davis contends that legal positivism commits him to "moral relativism" and an "approach" to constitutional interpretation associated with "the idea that the Constitution has a fixed meaning, and the view that it comprises a set of rules to be strictly followed" (p. 28).

Much of the book consists in reviewing Rehnquist's record in light of Davis's unsympathetic portrait of the justice's legal philosophy. In chapter 3, for example, Davis does not counter or "disprove" allegations that Rehnquist's opposition to the Burger Court's expansive reading of the Equal Protection Clause reflects a "mean-spiritedness," "the lack of awareness of an upper middle class white male," or a "political agenda" (p. 63). Instead, she describes Rehnquist's record as a generally coherent expression of his deference to majoritarian democracy, moral relativism, and legal positivism. And so it goes in succeeding chapters, examining Rehnquist's positions on the First Amendment, traditional and nontraditional property, and federalism.

In the final chapter, "Legal Positivism, Federalism, and Rehnquist's Constitution," Davis switches gears and provides a superficial overview of the 1986 confirmation skirmish over his elevation to chief justice. She correctly argues that as chief justice, Rehnquist is not likely to moderate his views or "retreat from his commitment to federalism" (p. 203) and then speculates on what the Rehnquist Court might do to the fabric of constitutional law.

When finally assessing Rehnquist's record, Davis concludes much as she began. She does, though, reveal some inconsistencies in his "literalism" and "strict constructionist" approach to constitutional interpretation. When defending "state autonomy" against an overreaching Congress and national government, Rehnquist abandons a close reading of the Constitution for a "structural analysis." Had Davis examined Rehnquist's opinions on separation of powers, she would have also found that they do not square well with all that she attributes to Rehnquist's legal positivism. Nor does Davis persuasively explain Rehnquist's defense of "state autonomy" in some but not all areas of constitutional law. Moreover, while Davis argues that Rehnquist's defense of "state autonomy" is buttressed by his view that courts generally ought to defer to majoritarian democracy and that federalism "is a means of protecting the democratic process" (p. 152), she does not fully explain his willingness to thwart the operation of majoritarian democracy in the national political process but not when it comes to state and local politics. That is because Davis's narrow framework and focus on Rehnquist's judicial philosophy exclude consideration of his broader political philosophy and vision of constitutional politics. And the latter is where others must ultimately turn when coming to terms with Chief Justice Rehnquist's judicial philosophy.

DAVID M. O'BRIEN

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At The Margins: Presidential Leadership in Congress. By George C. Edwards III. New Haven: Yale University Press, 1989. 224p. $24.00.

Since the emergence of the two-party system, presidents have intermittently been cast in the role of legislative leader. Certainly, in the twentieth century the prevalent expectation has been that the president act as chief legislator. Instances of exceptional executive success in securing congressional support for major policy initiatives have provided precedents that are all-too-often regarded as norms for legislative leadership. These precedents include Woodrow Wilson's first-term progressive policy successes, Franklin Roosevelt's New Deal legislative leadership (particularly during the proverbial "first hundred days"), Lyndon Johnson's prolific Great Society enactments, and Ronald Reagan's initial successes concerning budget, tax, and defense legislation.

Conversely, another stream of both scholarly research and political experience argues that such leadership expectations are excessive and that the stellar precedents noted above represent rare and unusual convergences of opportunities and abilities. *At The Margins* focuses on the apparent paradox between the mythic expectations for presidential leadership and the less impressive performance of the past seven presidents. Indeed, Edwards poses this paradox as an important feature of the U.S. political culture which forces presidents "into an active leadership role, and at the same time complicates his efforts by failing to provide reliable support and insisting that he be all things to all people" (p. 15). Consequently, the central question is, "What if presidential leadership has less potential than holders of the conventional wisdom believe and the president actually operates at the margins in obtaining support in Congress?" (p. 3). The book does not derogate the impact of leadership but rather assesses how much and under what conditions it can be exercised.

Edwards skillfully develops a contingency analysis of presidential leadership in Congress. He emphasizes the importance and variability of what he calls "the strategic position" of the president, which derives from three prime sources of presidential influence on Congress: nominal party leadership (and attendant partisan support), public opinion, and legislative skills. Separate chapters examine the character and variability of partisan support in Congress, how public opinion standing and electoral circumstances provide a context for presidential attempts to mobilize and convert support within Congress, and the methods and the limitations of presidentially exercised skills of legislative persuasion. Each of these chapters presents a wealth of data on congressional behavior with breakdowns to indicate variations between parties, chambers of Congress, and philosophical-regional factions within parties and presidential administrations. Indeed, full computation of this data involved the review of several thousand roll calls, the initial computation of four different indexes of congressional support, and the coding of over seventy-two thousand index scores (p. 25).

Edwards treats established wisdom and customary explanations for how, how much, and why presidents exert leadership on Congress as theoretical propositions to measure against his data. Although he finds a kernel of truth in each of these propositions, he nonetheless concludes that the resources and tools available to presidents for use in persuading Congress to follow their leadership are only of marginal utility, effectiveness, and reliability. In other words, presidential leadership efforts operate at the margins of the congressional decision-making system.

The exposition of these findings is a rich and complex amalgam of quantitative data, contextual analysis, and historical perspective. Arguments and analyses are well illustrated with presidential examples from Eisenhower through Reagan. In one of his best chapters, Edwards analyzes the validity of the "two presidencies" perspective. He argues that presidents do *not* enjoy special advantages of bipartisan support or congressional deference regarding foreign affairs but rather that Eisenhower atypically experienced a temporary success in securing support for his internationalist foreign policies from a liberal congressional coalition that crossed party lines (p. 68).

Similarly, his analysis of the character and possible import of mandate elections is insightful and nuanced. Mandate elections are not simply a matter of magnitude but a "combination of change, a substantial defeat of the leading opponent, and a sense of new directions in policy" (p. 161). Unfortunately, while winning a mandate election can provide a president with leadership and governance assets, campaign strategy seems to require the presentation of only general goals and images, thereby discouraging candidates from imparting a clear sense of policy direction.

At the Margins is a richly textured analysis of a governance relationship that is crucial to the U.S. political system. While eschewing a heroic view of the president, it nonetheless recognizes the importance (and variable impact) of individuals on the course of public affairs. In identifying the contingencies that present opportunities (as well as obstacles) for presidential leadership in Congress, Edwards emphasizes that presidential leadership is neither impossible nor merely marginal. Rather, if presidents are skilled in recognizing and exploiting opportunities in their changing environment, the exercise of leadership—even if only at the margins—can turn opportunities into significant accomplishments.

MARGARET J. WYSZOMIRSKI

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Political Ambition: Who Decides To Run for Congress? By Linda L. Fowler and Robert D. McClure. New Haven: Yale University Press, 1989. 237p. $25.00.

Read descriptively, *Political Ambition* is intelligent, instructive, and often entertaining. Read theoretically, it is disappointing. Let us treat the descriptive effort first.

In early 1984 a popular twenty-year veteran of the House of Representatives, Barber Conable, announced his intention to retire, thereby setting in motion a scramble for the right to represent New York's 30th Congressional District. Fowler and McClure alertly seized the opportunity to explore in-depth the process by which political personalities and parties would maneuver to control the succession. The authors were as interested in the might-have-run candidates as they were in the Republican and Democratic candidates who, in the end, contested the election. In their words, "What voters hear in November and how persuasive the messages turn out to be depend in large part on who decided many months earlier to run and who chose to remain on the sidelines" (p. 235).

In this largely ethnographic recounting of who ran, who did not, and with what consequences the authors time and again uncover and intelligently (if perhaps repetitiously) describe a good many features of the congressional recruitment process. For example, the contest turned out to reveal much that is important about gender and politics, the authors make good use of how women's issues and interests expressed themselves, a story enriched by the fact that a potential woman candidate was among the self-eliminating but then returned two years later to unseat the incumbent against whom she had earlier decided not to compete.

Elsewhere and throughout, the authors subtly tell the story of how campaign finances, actual and prospective, influence not just the outcome but the nature of the links forged between local politics and national power centers. There is good stuff here not just about campaign funds but more generally about federalism as it plays itself out in congressional elections. Confronted with the organizational weaknesses resulting from federalism, it takes an unusually ambitious politician to link together the local, state, and national elements necessary to wage a winning campaign: "By force of will these ambitious politicians build their own federal campaign apparatus" (p. 197). That another author might have substituted for the adjective "ambitious" any of several others—effective, energetic, talented—hints at the difficulties Fowler and McClure face when they shift from informed description to theory building.

The book falters in its effort to account, theoretically, for political ambition. In this failure the authors, of course, are not alone. Political ambition—indeed, ambition in corporations, universities, volunteer organizations, and on and on—is the ever-so-subtle interplay of shifting self-images interacting with shifting opportunity structures. It is too often told as the story of "what might have been," a temptation the authors, finally, cannot resist: "As we survey events we cannot help speculating about what might have been" (p. 236); and they proceed to do so, uselessly. Thus, they discover, following countless scholars before them, that theory building in the face of the counterfactual is an elusive enterprise.

Consider the "facts" of ambition as presented. In the 30th Congressional District of New York in 1984 the victorious candidate is described as having a "burning ambition" to serve in the House; the authors repeatedly return to this singular fact to explain his victory over a candidate whose ambition they characterize as "indeterminate." Then add an unsure, even insecure, noncandidate in 1984 for whom, two years later, "nothing had altered except her own ambition, and her willingness to act on it" (p. 204). This now-ambitious candidate unseated the very incumbent whose burning ambition had taken him to

Congress but now "no longer had the fire in his belly needed to defend a House seat against a determined challenger" (p. 219).

Perhaps so; but ambition, so elucidated, becomes, necessarily, an after-the-fact explanation—winners score higher on the ambition scale than losers. Political science has waited a long time for a theoretically compelling account of political ambition and its effect on democratic accountability. The title of the work under review notwithstanding, it will have to wait a bit longer. In the meantime, we can always reread the *Federalist Papers,* wherein is set forth the key argument that without political ambition there can be no democratic accountability. This proposition—very lightly treated by Fowler and McClure—gives reason to our effort to understand political ambition in its sources, its distribution, its consequences.

KENNETH PREWITT

Rockefeller Foundation

Nominating Presidents. By John G. Geer. New York: Greenwood, 1989. 176p. $37.95.

John Geer's two objectives are to examine whether voters in primaries are qualified to choose presidential nominees and to provide systematic information about voters in primaries. To fulfill those objectives, Geer examines the extent to which primary voters are representative of the party following, the level of participation in presidential primaries, information levels of primary voters, the bases of vote choice in these elections, the media's role in selecting presidential nominees, and the impact of primaries' rules on voter behavior. Having examined these six aspects of the presidential selection process using media exit polls, American National Election Study data, and aggregate data compiled from state records, the census, and other published sources. Geer then presents a proposal for reform of the nominating system.

Some of his conclusions are at variance with prior research, in part because different sources of data are used and also because he defines key terms differently. For example, in examining the representativeness of primary voters, Geer concludes that they are unrepresentative of the *party following;* but he defines the party following as those who usually vote for the party (i.e., all party identifiers who vote in the general election regardless whom they voted for) and independents and defectors from the opposite party who voted for the party's presidential candidate. Geer concludes that primary voters' unrepresentativeness resides in their being less educated and having lower incomes and more moderate political orientations than the party following in the general election.

Rather than lamenting low turnout in primaries, Geer argues that greater rates of participation are achieved through the primary process than through any other nominating process. He focuses on explaining variation in turnout levels across presidential primaries within an election year and across time. Geer redefines the problem of turnout, focusing on turnout in each party's primary by the party's registered voters rather than as a proportion of voting age population. As the author acknowledges, the effects of voter registration laws are therefore ignored; although scholars generally agree that these are significant in their impact on turnout. Furthermore, turnout patterns in open primaries cannot be assessed. Geer's analysis also ignores the impact on participation patterns of an incumbent seeking renomination and the effects of primaries held at the same time for other offices.

The rules governing primaries, Geer argues, affect the ability of voters to make good choices. The rules, not voters' decisionmaking abilities, result in the selection of candidates with narrow bases of support.

Geer concludes this book with a proposal for reform of the presidential nominating system that rests on two assumptions: (1) primaries are not going to be abolished, and (2) the election system should be structured to select candidates with broad support. His proposal is for a system of regional primaries—a set of super-Tuesdays—separated by sufficient time to permit both voters and candidates to digest the results of each and to adjust to the implications of each outcome. Each primary would be composed of a set of states selected to provide ideological balance, have similar numbers of delegates at stake, be as contiguous as possible, and be balanced in demographic composition. Geer does not adequately address the problem of getting such a nominating system adopted.

While alternative formulations of key concepts can be developed and would result in different conclusions from those reached by the author, Geer's book is an original and well-executed study of several aspects of the presidential nominating process and a useful contribution to our knowledge of the contemporary nominating process.

M. MARGARET CONWAY

University of Florida

Taming the Bureaucracy: Muscles, Prayers, and Other Strategies. By William T. Gormley, Jr. Princeton: Princeton University Press, 1989. 269p. $29.50.

Taming the Bureaucracy: Muscles, Prayers, and Other Strategies is an addition to the growing literature on the relationships between public bureaucracy and other aspects of the U.S. political system. Broadly speaking, this literature, which includes contributions by several leading twentieth-century public administration scholars such as Frank Goodnow, Dwight Waldo, Herbert Kaufman, Frederick Mosher, and Emmette Redford, can be divided into two conceptual frames: one takes bureaucracy as the unit of analysis and considers the impact of other components of the political system on it, and the second examines the interaction between bureaucracy and political processes or institutions. Judith Gruber's *Controlling Bureaucracies: Dilemmas in Democratic Governance* is a recent example of the first approach; whereas Morris Fiorina's *Congress: Keystone of the Washington Establishment* and Theodore Lowi's *End of Liberalism* are contemporary examples of the interactive perspective. Gormley locates *Taming the Bureaucracy* firmly in the genre viewing public bureaucracies as units of government to be acted on by legislatures, chief executives, courts, interest groups, and the public. Little consideration is given to how the strategies and efforts to tame bureaucracy affect those who would do the taming.

Within the limits of this conceptual framework, Gormley's book "is about solutions to the bureaucracy problem" (p. 6), particularly the familiar issues posed by administrative discretion and authority. His purpose is to move the polity away from mindless bureau bashing and toward thinking strategically in developing approaches that are "purposive, comparative, and contextual" (p. 6). Herein lies the book's major contribution. Gormley convincingly argues that the variety of controls over bureaucracy available to the political system must be thoughtfully matched to the specific character of the bureaucratic problem being faced. For example, privatization surely has some uses, but it cannot be used to resolve the problems associated with every public bureau everywhere.

Among the strategies Gormley recommends are coercive controls ("muscles"), such as legislative vetoes and sweeping court injunctions; catalytic controls ("prayers"), which "require the bureaucracy to act but do not predetermine the nature of the bureaucracy's response" (p. 12), such as ombudsmen and environmental impact statements; and horatory controls, such as legislative audits, which fall in between the other two types.

The main portion of the book is devoted to discussing these strategies under the titles "Interest Representation," "Due Process," "Management," "Policy Analysis," "Federalism," and "Oversight." Although much of the material in each of these chapters will be commonplace to political scientists with interests in bureaucratic politics and public administration, Gormley makes a stellar contribution by integrating a wide variety of state-level studies into his general discussion. One could quibble with some of the content of these chapters, but only "Due Process" is seriously flawed. Confining the discussion of judicial involvement in public administration to due process misses much of the major constitutional and legal development of the very period Gormley emphasizes, that is, the 1960s to the present. For example, it ignores the parallel growth in equal protection, which not only led to the desegregation of prisons and public schools but also promoted fundamental changes in public personnel systems. It also neglects the revolution in public administrators' liabilities for constitutional torts. Moreover, the discussion leads to such peculiarities as encompassing federal judges' strategies for forcing prison reform in the states but not in federal facilities, which are covered directly under the Eighth Amendment. In fact, due process cannot account for the constitutional rights to freedom of speech, assembly, exercise of religion, travel, and privacy that individuals may assert in federal court against federal bureaucracies.

The book is written in an engaging manner, displaying wit and making comical cinematic allusions. Overall, it usefully augments the literature on bureaucratic politics in the United States.

DAVID H. ROSENBLOOM

Syracuse University

Contemporary Federal Policy toward American Indians. By Emma R. Gross. Westport, CT: Greenwood, 1989. 144p.

Formulating American Indian Policy in New York State, 1970–1986. By Laurence M. Hauptman. Albany, NY: State University of New York Press, 1988. 215p.

There is an unfortunate gap in our understanding of how American Indian policy is formulated. In the vast body of literature on the subject, only a few studies have reached beyond describing historical events to place the development of American Indian policy in any kind of theoretical framework. By and large, Indian policy is seen as a historical topic, outside the mainstream focus of public policy analysis.

Emma Gross's *Contemporary Federal Policy toward American Indians* and Laurence M. Hauptman's *Formulating American Indian Policy in New York State,* however, are attempts to close the gap in our understanding of Indian policy and treat the subject as one worthy of proper analysis. Of the two studies, Gross's analysis is the more focused and theoretical. From the beginning, she utilizes the "classical," or constitutional, and interest group models of public policy formulation to find out how much influence American Indians have in the policymaking process. Until the 1970s, Indian input was virtually nil except to rely on litigation to block anti-Indian legislation or to pursue tribal land claims. It could be argued that the true constituencies of the Bureau of Indian Affairs and Congress, at least in regard to Indians, were the railroads, settlers, mining companies, Christian reformers, and sundry other non-Indian groups. Policies favorable to Indians were made largely through the court system and only to respect constitutional allowances or limitations.

According to Gross, Indian policy formulation changed during the 1970s from the constitutional to the interest group model. The Civil Rights movement of the 1960s created a "liberal shift" in government. Indian aspirations were given credibility and an unprecedented degree of representation. The 1970s saw the formation of several Indian advocacy groups that gained access to the policymaking process. Minority problems were on the public agenda and Indians and Indian advocates began to place more emphasis on legislation than on litigation. The relationship between the federal government and American Indians became less adversarial and more cooperative. This shift in relationships was probably the result of a change in the way policymakers viewed American Indian land claims and demands for self-determination. Very likely, these Indian demands were seen not as efforts to gain constitutional sovereignty (which they were) but as conditions for economic development on the reservations.

Standing in stark contrast to Gross's findings are those of Laurence Hauptman in his study of Indian policy in New York State. According to Hauptman's well-documented and insightful study, American Indian–New York State relations today are "in crisis" (p. 111). Although New York does attempt to provide services to American Indians under its constitutional mandate, the government basically views Indian tribal land claims and sovereignty as antithetical to state interests. State objectives dictate policy. Of primary importance to New York officials is the protection of the state's land base and its jurisdiction over criminal and civil matters. Even Indian economic development is seen as giving the tribes a right to exploit New York resources. Moreover, Indians are generally seen as barriers to state development. Hauptman argues that the relationship between Indians and New York State is basically adversarial and that unless state officials shift their views and regard Indian land claims as legitimate, there will be no room for cooperation in the policymaking process.

Taken together, these two studies offer a broad picture of Indian affairs at the present time. They demonstrate not only the great differences between federal and state policy formulation but how both federal and state policymakers view Indian aspirations. The federal government, at least at this point in time, seems less threatened by the demand for Indian tribal sovereignty than do the states. Why this situation exists has yet to be explained.

TOM HOLM

University of Arizona

The Politics of Regulatory Change: A Tale of Two Agencies. By Richard A. Harris and Sidney M. Milkis. New York: Oxford University Press, 1989. 331p. $34.50 cloth, $13.95 paper.

When President Reagan took office in 1981, one of the promises he made was "decisively" to reverse past regulatory trends. A number of authors provided interim assessments of the Reagan program for regulatory reform but only recently have any full evaluations of the administration's efforts been possible. This book moves a long way toward that end by placing the Reagan era reforms in a political and ideological context designed to illuminate to what extent the regulatory process and its outcomes were truly altered. To do this, the authors initially review the history of regulation during this century, focusing on two periods: the New Deal and the early 1970s. They use these two periods to illustrate their approach (spelled out in chapter 2) for "measuring" regulatory change in terms of the ideas, policies, and institutions that characterize any specific period. This also serves to set a baseline to which the Reagan era changes can be compared.

Harris and Milkis suggest that the recent period of social regulation differs substantially from the New Deal regulatory regime due, in part, to its focus on reforming how people lived. The era of social regulation, they claim, was founded on a number of anticonsumerist, anticapitalist ideas advanced by members of the New Left. This contrasts sharply with the New Deal efforts "to reform the capitalist system in order to preserve it" (p. 62). This argument seems a bit strained at times, but the authors' catalog of new policies and of the regulatory institutions created to enforce them add a measure of credence to their claim that the era of social regulation reflects more than merely incremental change when compared to the New Deal era.

The book then moves on to consider the effects by the Reagan administration to reform federal regulation, this time evaluating to what extent the policies, process, and outcomes of the Reagan era actually differed from those developed during the preceding 10 or 12 years. Through separate examinations of the Federal Trade Commission (FTC) and Environmental Protection Agency (EPA), Harris and Milkis conclude that Reagan's efforts decisively to reverse previous regulatory trends were effectively frustrated by the inertial forces endemic to our political system, as well as the lack of a coherent ideological foundation for the administration's application of regulatory relief. Their contrast between the ideologically based, and therefore frequently successful, reform at the FTC and the apparently ad hoc and unsuccessful application of regulatory relief at the EPA is especially noteworthy. The same is true of how the authors highlight the centralizing effect of the administration's resort to administrative change. Such a strategy became necessary when their early efforts to rewrite such statutes as the Clean Air Act from the ground up were stymied by a broader set of political actors and institutions less willing to overthrow the existing regulatory regime.

The conclusion that the administration's efforts to establish a new regulatory regime were unsuccessful follows neatly from the authors' initial definition of a regulatory regime and what we expect to see with a change of regimes. Yet some of their other evaluations are not as easily accepted. For instance, the claim that the era of social regulation is an example of a regime change isn't wholly satisfactory. Not only are the ties to New Left ideology less concrete than they might be, but alternative explanations are not considered. A strong case might be made that while New Deal regulatory policies were predicated on reforming some of the less wholesome aspects of unbridled capitalism, the policies reflected in the more recent "social" regulation simply extend the "economic" reforms to include market failures (pollution) and inadequate information (workplace safety).

Another claim that prompts a desire for more data, along with an examination of alternate hypotheses, is the claim that the Reagan administration's policy changes are best seen as either short-lived administrative adjustments or incremental changes within the existing regulatory structure. Given the focus of the case studies on agencywide behavior, these conclusions are reasonable. However, this methodology fails to evaluate to what extent the creation and application of a regulatory central clearing function within the Office of Management and Budget (OMB) might have fundamentally altered agency outputs. Furthermore, while the authors note the control that OMB can now exercise over agency regulatory agendas thanks to EO 12498, they do little to shed light on the effect such a potentially far-reaching change has actually had on regulatory outcomes.

While this book leaves some questions inadequately answered, it nonetheless deserves the attention of anyone concerned with regulatory reform and the impact of such reform on the broader political system. The theoretical framework offered by Harris and Milkis appears to be quite useful and clearly demonstrates the value of looking beyond statistics and other discrete measures of change in order to evaluate the broader system—in this case ideas, policies, and institutions. Regardless of whether one accepts the argument that the era of social regulation marks a distinct break from the New Deal regulatory era, it is clear that the Reagan era reforms have stopped somewhat short of their goal. Just how short—and how long they will last—must be answered by looking into the details of administrative reform and the extent to which it is either institutionalized or in some way revised during the Bush administration.

William J. Pielsticker

University of Michigan

From Margin to Mainstream: American Women and Politics since 1960. By Susan M. Hartmann. Philadelphia: Temple University Press, 1989. $29.95.

From Margin to Mainstream, as the title suggests, is an extended historical essay detailing the tactics and strategies employed by women to become more central to U.S. politics. After placing the work in historical perspective through an introductory chapter on the post-suffrage era, the book chronicles the contributions of individual women, women's groups and organizations, and the female electorate to the development of public policy from the 1960s to the present. Interesting details are provided on individual women, such as Rosa Parks, Fannie Lou Hamer, Jessie Lopez de la Cruz, Delores Huerta, Bella Abzug, and Phyllis Schlafly. The roots of women's organizations are traced back through the civil rights and student struggles of the 1960s and grass-roots antipoverty groups of the 1970s. The emergence of the gender gap between male and female voting patterns is analyzed in light of the issues and events on which it is most visible.

The focus is on how women's concerns have been brought to the mainstream of political life. This is not to imply, however, that women's interests are treated as a monolithic block. Rather, the policies pursued both by individual women and groups of women can be quite different. This is illustrated by the opposing sides taken by women's groups on issues such as abortion, the election of liberal males versus conservative females, and the adoption of particular political tactics and strategies.

Useful as a reference work, this volume is suitable for inclusion in graduate courses in political science, history, and women's studies. The reader is introduced to the major background sources in this area, and a solid orientation is provided for those wishing to do further research. The detailed bibliographic essay is also helpful in this respect, as are the author's suggestions for more studies on the history and development of women's organizations and the Women's movement in general.

One problem with this volume is that despite taking an historical approach, not all issues are followed systematically through each chapter. In this respect, the inclusion of a chronology would have been helpful.

While this is not an exhaustive treatment, important public policies on the national agenda, such as abortion rights, pregnancy discrimination, equal pay, and educational equity are discussed; there is less coverage of state and local issues. One problem with the study of women and politics, however, is that advances are made somewhat slowly while previous gains can be rather quickly eroded. Very recent developments in public policy areas affecting women (e.g., reproductive rights and affirmative action) seem to contradict the book's optimistic conclusions on the strength and direction of women and politics.

MICHELLE A. SAINT-GERMAIN

University of Arizona

Representing God in Washington: The Role of Religious Lobbies in the American Polity. By Allen D. Hertzke. Knoxville: University of Tennessee Press, 1988. 260p. $29.50 cloth, $14.95 paper.

This is a fine book, sure to prove provocative for scholars of interest group politics as well as for those interested in religion and politics generally.

The importance of the book is demonstrated by Hertzke's major thesis: that religious lobbies provide a significant corrective to the U.S. pluralist system (and a refutation to the critics of interest group politics) through their role in representing nonelites and unpopular attitudes. He makes two other interesting and uncommon contentions. He insists that influence is a two-way street, the organization and norms of the Congress and U.S. political system influencing religious lobbies and the churches fully as much as they in turn influence politics. He also suggests that the effect of prolonged play in the political arena is to moderate the religious propheticism and radical, "fringe" politics of many religious organizations, including the Moral Majority.

Hertzke's book is important to interest group scholars because he attempts to deal with the field's most criticized deficiencies. The first is its common failure to determine whether the generals have any troops. Through the use of survey data and an examination of organizational structure, Hertzke makes an imperfect but creditable attempt to ascertain to what degree church members share the political positions of their leaders and how they can influence, if not direct, their leaders' political actions.

Too often, interest group research also fails to move beyond input to output. Scholars examine an interest group's organization, goals, and strategies without showing whether or not it then actually accomplished anything—or, if it did, *how* it did. Hertzke examines the equal access bill of 1984, which asserted the right of religiously oriented student groups to meet in their public schools, just as do other student groups. He demonstrates the primary role religious lobbies played not only in its passage but in writing the regulations for its administration.

He also uses his equal access study to examine how and why religious lobbies succeeded in this case and comes to a rather unsettling discovery: religious interest groups seem most likely to achieve their goals when they lose their narrowly religious focus; that is, religious lobbies are most likely to be successful when they can frame their goals not in narrowly sectarian or grandly prophetic terms but in the more widely accepted, secular terms of normal U.S. politics—"civil rights" rather than "religious rights." Concomitantly, they are successful when they focus on issues that reflect the fears or needs of the public in general rather than those evoked by a particular denominational creed or religious ideology.

This leads to my major questions, which do not so much involve Hertzke's book as they do the role of religion in U.S. politics. Hertzke essays the influence of religious groups in regard to such major political issues as our foreign policy in Latin America and the recent fate of social welfare policy. In the end, though, he can firmly trace religious influence on legislation only in the rather insignificant equal access bill. He also asserts (and, on the basis of my own research into religion and politics, I agree) that religious groups are most successful in politics when they are least "religious." I am forced then to wonder how much influence religion does have on U.S. policymaking if its influence can only be demonstrated on minor issues and by the adoption of politics-as-usual terminology and goals.

Despite these questions, Hertzke's book is a very welcome addition both to interest group politics and to religious-political studies. There have been only two extended studies of church lobbying, by Ebersole (1951) and Adams (1970), both preceding the intense activism of Catholics and fundamentalists in the late 1970s and 1980s. Wald's *Religion and Politics* (1987) and Reichley's *Religion in American Public Life* (1985) present good general overviews of religious-political relations in the United States but necessarily paid scant attention to religious interest group activities.

Interest group studies, in turn, virtually ignore the political role of churches, in part because until recently modern political science as a field ignored religion. The Civil Rights and Antiwar movements, the abortion debate, the Catholic Bishops' letters on nuclear war and the U.S. economy, and the emergence of the Moral Majority all helped to change that. Allen Hertzke's book, *Representing God in Washington,* is a significant addition to what is now a growing body of serious scholarly studies of the interstices between religion and politics in the United States.

MARY T. HANNA

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Thirty-Second Politics: Political Advertising in the Eighties. By Montague Kern. New York: Praeger, 1989. 237p. $45.00 cloth; $16.95 paper.

From Whistle Stop to Sound Bite: Four Decades of Politics and Television. By Sig Mickelson. New York: Praeger, 1989. 186p. $37.85 cloth, $14.85 paper.

Montague Kern has made an important contribution to our understanding of the role television commercials play in the political process. She started her research in 1984, asking two questions: How important have these commercials become "in the total network informational mix" viewed by U.S. voters? and What impact do these commercials have on the campaign process? Now she has some answers.

She taped commercials in four states between 6:00 P.M. and 9:00 P.M. during the last 10 days of the 1984 election; interviewed numerous campaign managers, consultants, and candidates; and amassed a file of 450 "media-consultant philosophical statements," which she then analyzed. She interviewed television news station directors in North Carolina and Indiana (the locales for a number of her case studies). She examined 569 commercials from statewide and congressional races in 1986 to determine whether the patterns she discovered in the 1984 races were carried further.

One purpose of her research was to see whether political commercials had changed significantly since 1972, when research by Patterson and McClure suggested that the basic role of television commercials was informational and that "mindless" and "emotional" ads were little relevant. One very important decision she made was to incorporate semiotics into her analysis of the commercials. Thus, she attempted to discover how commercials conveyed meaning to people and how aesthetic factors such as camera angles, shot selection, editing, and the use of symbols impinged on viewers. In this respect she analyzes a number of commercials and in some cases even provides their scripts.

Her conclusions may not startle people who have paid any attention to how commercials were used in the 1988 campaigns; but she provides strong evidence that television commercials now dominate campaigns, that they have changed from being informational and issue-oriented to being emotional and symbolic, and that negative advertising works. She offers illuminating case studies of the 1984 presidential race, the 1984 Hunt-Helms race in North Carolina, and a number of races in Indiana and Southern states.

Although this is a scholarly work, it will be (or should be) required reading by all political campaign managers and media consultants; for it is full of information and insights, all supported by evidence, about what works and what doesn't in political campaigns. In a number of respects, Kern's findings are distressing. Among other things, she suggests that "advertising can overwhelm news coverage," which means we cannot assume that television news will provide correctives to the excesses of political commercials.

She asks, "Can one's civic responsibility be adequately nurtured while alone, viewing 30-second political messages on the flickering screen?" The answer is quite evident. She also points out that these commercials tend to appeal to individuals as isolates and not to any sense they may have of themselves as members of economic or social classes, parties, or other institutions "above the family level." One wonders, Are these commercials causes of political alienation or reflections of it?

Sig Mickelson has written a much different kind of book; but he ends up at the same place, lamenting, it would seem, the role that "sound bites" and political commercials now play in the electoral process. As his subtitle indicates, his book has a historical cast to it and recounts the role he played in the evolution of television for forty years. He was a major figure in the television industry and tells us, firsthand, about how television started getting involved with the political process.

He is a wonderful raconteur, and his book is an extremely lively and fascinating one. But he is also a critic and comes to disturbing conclusions about the impact television is having on politics. He believes that communication tools are neutral and how we use them crucial. On the other hand, he mentions that television "is brilliantly effective in delivering images and symbols" but often strikes out when it tries to deliver information. How can this medium be expected, then, to function effectively as an information delivery system and help people make rational decisions about candidates?

He also points out how television networks and stations are driven to pursue high ratings, which lead to large profits. There is, it would seem, an imperative to the broadcasting industry, at least as it is organized and financed in the United States. And this economic imperative makes television stations and networks desperate to find and broadcast material that will attract viewers.

He speculates about what impact "the triumph of imagery over facts, of symbols over character, and of show business over rational argument" has had and whether "irreparable damage has been done to the electoral system" by television. Is television, somehow, debasing the electoral process? If he wants an answer to this question (and it will confirm his worst fears), he would be well advised to read Montague Kern's superb book.

ARTHUR ASA BERGER

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The Court Versus Congress: Prayer, Busing, and Abortion. By Edward Keynes. Durham, NC: Duke University Press, 1989. 400p. $49.50 cloth, $16.95 paper.

Judicial review is a well-known and much-admired U.S. political invention, a practice so well regarded that it has evoked imitation abroad. The awkward fact that our federal constitution provides no clear justification for judicial invalidation of legislative and executive action is also widely apprehended and has provided grist for decades of scholarly and political debate. In Congress this debate tends to be episodic and located in conflicts over particular decisions of the Court that are unpopular with both Congress and voters. Bills are proposed—but seldom adopted—that would place obstacles in the path of litigants who seek to vindicate certain types of constitutional claims in the federal courts. In academia, judicial review has been a preoccupation of constitutional scholars for generations; there is little consensus on where judicial review fits in our scheme of constitutional democracy. How can one make sense of this endless controversy?

The Court Versus Congress guides readers through the confusing institutional history of judicial review and provides some assistance in locating the positions of academics who have addressed the issue. The author begins, not surprisingly, with the founding period. He reads the evidence to suggest that the framers and the ratifiers understood the Supreme Court to have power to declare both federal and state legislation unconstitutional. No other interpretation is consistent with the themes of national government, shared powers, and constitutional supremacy that gave definition to the founding effort. Most of the rest of the book is concerned with how Congress and the Supreme Court have dealt with the ambiguities of the founding document. Congress, Keynes concludes, has consistently acknowledged the necessity for allowing constitutional claims a hearing in the federal courts: "Since 1789 Congress has not denied litigants seeking to vindicate their constitutional rights access to some federal forum" (p. 309). Proposals to limit judicial jurisdiction over constitutional claims should be analyzed as warning shots across the Court's bow. Keynes brings home this point in chapters on three of the hot constitutional conflicts of our time: school prayer, court-ordered busing, and abortion rights.

The chapters on prayer, busing, and abortion are detailed enough to provide a sense of the politics of judicial review in congressional debate. At the conclusion of each of these chapters, Keynes describes his own position in favor of an expansive interpretation of the scope of judicial power. The detailed chapters thus return the reader to the overall scheme of the book: justification through historical demonstration, both of the founders' original intent and of the pattern of practice in Congress over two centuries.

The Court Versus Congress is a useful reference book for constitutional teachers. The writing is clear, and the argument well organized; individual chapters stand on their own comfortably. The author is modest in presenting his own interpretation of the founding and current controversies. His primary concern is to present the positions of those who have weighed in on the subject of judicial review in political life and in academia.

Those whose views Keynes describes as part of the academic, judicial, or legislative mainstream are almost entirely male, it should be noted. The predominance of male voices, especially in a book containing a chapter on abortion, is really quite striking. Indeed, out of 262 individuals referenced in the index, only four are women. Is Keynes a sexist, or has judicial review as seen through the lens of politics been a peculiarly male concern? I see in this pattern a discouraging reminder that Congress and the courts have historically been male preserves; the leading spokespersons in these institutions have been even more overwhelmingly male. Contemporary academia is perhaps less dominated by men. What explains the absence of female voices drawn from this sphere? The answer may lie in Keynes's purposes in writing this book and in the gendered pattern of scholarship related to the constitution. Keynes reports on academic theorizing about judicial review from the vantage point of congressional policymaking. Only those whose contribution speaks to the debate over broad or narrow judicial power are relevant. Those who would set the debate over judicial review in a broader political frame are beyond the scope of this book. Those who argue that the more interesting constitutional dialogues are those that arise from the experience of marginalized populations are themselves marginalized in this analysis. Had Keynes tried to work in these voices, the presentation would have been much messier and the path of analysis much harder to follow. The book instead stays within the constitutional canon, which is its organizational strength and its conceptual weakness.

DORIS MARIE PROVINE

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In the Name of War: Judicial Review and the War Powers since 1918. By Christopher N. May. Cambridge, MA: Harvard University Press, 1989. 370p. $29.95.

Conflict or Codetermination? Congress, the President, and the Power to Make War. By Marc E. Smyrl. Cambridge, MA: Ballinger, 1988. 180p. $26.95.

The U.S. constitution has come under seige in U.S. political discourse. Controversy ranges over its imprint on contemporary domestic politics, its impact on U.S. foreign and defense policies, its license and imprimateur on the U.S. judiciary. In particular, the separation-of-powers doctrine, like a storm cloud that never quite dissipates, commands wary attention. What political and policymaking process did the framers mean to establish when they extended their open-ended invitation to play the rough-and-tumble game of checks, balances, and reciprocated struggle among the branches? How should each generation, as it confronts alternate challenges and dangers, define and apply truly constitutional standards in regard to the prerogatives vested in each branch, as well as in respect to interbranch constraint?

The debate is manifold and diverse, involving issues that are at once substantive and procedural, normative and practical. Disagreements arise over the nature and content of constitutional principle and doctrine, over rulemaking procedures that would guarantee constitutionally mandated outcomes, over policies and policy outcomes deemed to reflect constitutional values or practices, over mechanisms of government best suited to ensure constitutional accountability.

Liberals and conservatives debate the virtues and vices of an "imperial presidency," an "imperial Congress," and even an "imperial judiciary." James MacGregor Burns astutely observes that the framers have "outwitted us." Indeed, by dividing the powers of governance, they have prevented us from instituting a unitary federal authority, one that might haunt us in the form of a constitutional imperialism. While some authors may decry congressional activism, others may berate the steady accretion of executive power, and a growing number condemn judicial interventionism, nobody, as yet, has deplored all three together. At least, one may pick and choose among the three forms of activism to condemn.

In this sense, the original intent of the framers has been fulfilled. Diffused centralization was their faith, hope, and vision. Given the current state of federal affairs, their conception has been abundantly realized. Most observers tend to criticize big government from the perspective of what Kenneth M. Dolbeare, for one, calls "the dark side of the constitution," that is, institutional gridlock, the continuing demise of party discipline, the accelerating influence of special interests—in sum, from a perspective that stresses the advancing pace of federal paralysis, rather than its opposite. Let it be so. Our failures are the framers' successes. For the greatest dangers remain those posed by an unleashed central authority, in particular by a paramount executive branch operating before an obsequious Congress and a supine judiciary.

This, surely, is the moral of the rich set of diverse stories systematically researched and recounted in these two studies. Both contribute to our understanding of the fragility of constitutional practice under the war powers and the reasons why interbranch codetermination so often appears weakest when most needed. Together, they illuminate the dilemmas posed by constitutional warrants that would provide for the safety, security, and survival of the Republic by dividing the war powers. What a difficult—nay, perhaps impossible—assignment the framers bestowed on posterity: the need to sustain checks and balances before, during, and after war or conflict, when presidential obligation to lead is most powerful.

Christopher N. May portrays the painful rise of a doctrine of judicial review of war powers in the immediate aftermath of World War I, when the progressive and populist impulse for social reform and judicial activism ran counter to certain prevalent social and financial interests altogether willing to conjure up a red scare and the possibilities of a U.S.-style Bolshevik uprising. The result was an aggrandizement of legal authority by the executive branch that rendered separation of powers politically fictitious and, perhaps even more significant, judicially irrelevant. Citizens were subjected to authoritarian forms of federal control, including seizure of rail, cable, and communication services; political censorship through press and mail surveillance; bureaucratic regulation of consumer prices; severe injunctions against strikes; abuses against strikers; arbitrary enforcement of prohibition; and harrassment of "radicals" and dissenters in general. As May graphically demonstrates, the second Wilson administration was an imperial presidency that did more violence to Madisonian principles than any administration before or since, although neither the Harding nor Coolidge administrations moved any too quickly to eliminate abuses, especially those perpetrated by the repugnant Espionage and Sedition Acts of 1917 and 1918. And both the Congress and the courts went along even after two Supreme Court decisions —*Hamilton* v. *Kentucky* (1919) and *Ruppert* v. *Caffey* (1920)—enunciated a willingness to reassert the primacy of U.S. civil liberties and judicial scrutiny of executive war powers.

May briefly examines the history of judicial review of war powers up to the present. Courts tend to sequester themselves behind two versions of judicial discretion: a ceremonial hearing of a case culminating in automatic approval of executive policy or a perfunctory dismissal of a case on the grounds of its nonjusticiability. The repeated use of the political question doctrine leads May to cite Clinton Rossiter's observation, "The Court, too, likes to win wars." But as tragic internment of Japanese-Americans during World War II and court aloofness during the poisonous days of U.S. military involvement in Indochina serve to remind us, judicial abdication of its review authority may easily permit unconstitutional executive excess. But May entertains no illusion in this regard. He concludes his analysis of judicial review of the war powers by underscoring the important role of Congress: "If Congress, for whatever reason, is unwilling or unable to perform its assigned task, rights may be injured in ways that are immune [to] judicial redress" (p. 267).

Congress, on the other hand, also likes a good fight. As forays over Libya and in Grenada—and as the more recent military incursion into Panama—clearly reveal, the best kind of conflict is tactically brief, strategically successful, not too costly in terms of soldiers lost, and clearly demarcated between good and evil. If such conditions prevail, Congress is overwhelmingly predisposed to permit the president to proceed unhindered by it.

This political fact of constitutional life emerges in the analysis undertaken by Marc E. Smyrl of the War Powers Resolution of 1973, its political origins, its legislative history, its policy impacts, and its legal implications. This is a story told in the form of many stories: the rise of congressional codetermination in the aftermath of Vietnam and Watergate, the congressional battle over the terms of the War Powers Resolution itself, a review of the instances when the War Powers Resolution was invoked—or not invoked—by an executive branch bent on involving U.S. military forces in circumstances of imminent or ongoing hostility, and finally a survey of congressional responses to these situations and their eventual consequences.

Smyrl demonstrates that congressional capacity to constrain and censure the executive branch increases with cost, danger, and duration of conflict. But Smyrl also establishes that the history of U.S. military intervention abroad during the past two decades reveals a steady lowering of the casualty or danger threshold. Virtually with each passing crisis, congressional intrusion into the policymaking process has been provoked earlier, more dramatically, and at consistently lower levels of risk than had been true before. This lowered threshold of congressional involvement led to an attempt to renegotiate the terms of the War Powers Resolution ending in passage of the Multinational Force in Lebanon Resolution of 1983. Smyrl concludes that the War Powers and the Multinational Force in Lebanon Resolutions reflect a growing willingness on the part of the political branches to maintain a consultative framework that sustains open decisionmaking and national security.

EDWARD WEISBAND

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Congress, the President, and Public Policy. By Michael L. Mezey. Boulder: Westview, 1989. 255p. $42.00 cloth, $14.95 paper.

Mezey's book is an elaboration of the argument he presented several years ago in an article subtitled "The Futile Quest for Congressional Power" (*Congress and the Presidency* [1986]). It addresses important and provocative institutional questions that deserve a prominent place in all systematic thinking about U.S. national politics. Mezey's readers will want to digest Joseph Cooper's lengthy rejoinder, "Assessing Legislative Performance," in the same issue. I share most of Cooper's critical views about Mezey's argument.

Mezey's argument is that "the current relationship between the president and the Congress has adversely affected the capacity of the U.S. political system to produce good public policy" (p. xiii). The current system, he complains, too frequently produces either stalemate or poor policy. The fault lies in both constitutional arrangements (separation of powers) and in the current institutional arrangements within the two branches, particularly in Congress, whose collegial, representative, and decentralized character make it poorly suited for making good policy. The solution is a range of reforms that allow the president to dominate policymaking and relegate Congress to discussion, oversight, and parochial representation.

I think many in Mezey's audience, which will be large, will be puzzled by his book. It is strangely divorced from the considerable scholarship on the subject. We do not learn how his diagnosis differs from Huntington's or Sundquist's arguments, for example. Nor is there the systematic comparative analysis of policy characteristics and institutional arrangements that is sorely needed in this field.

A primary motivation for the book seems to be that the problems of stalemate and poor policy have become more severe in recent years. Most of Mezey's examples are from the Reagan administration; and midway through the book he explicitly argues that "over the past twenty years or so" stalemate and faulty policy have become more serious problems, which is attributed to a more fragmented Congress, weaker parties, flourishing interest groups, and a "growing dissensus" about government and policy in the general public (p. 110). Mezey's interpretation of these developments creates a sense of urgency for his proposed reforms. His case is not convincing.

Much of the blame Mezey places on the organization of Congress is misplaced or misspecified. There is little doubt that stalemate and highly compromised policy are due at times to an obstructionist subcommittee or freewheeling amending activity on the floor; but more responsibility lies with disagreement between the House majority, Senate majority, and White House. Implicitly, Mezey recognizes this in his discussion of reforms, which focuses on executive-legislative relations and largely ignores the internal organization of Congress.

As for misspecification, Mezey describes Congress as "fully evolved, fully decentralized, and now highly collegial" (p. 125) and therefore a hopeless source of good policy. In fact, neither chamber is fully decentralized and, technically speaking, Congress cannot be both fully decentralized and highly collegial: we cannot have both (fully) autonomous committees and (highly) collegial, floor-oriented decisionmaking. And even if a little hyperbole is functional, the analysis ignores or too readily dismisses developments in the 1980s and fails to distinguish between the two chambers. The House has managed to develop a fairly efficient, centralized process on matters on which House Democrats could cohere, in part because of the defensive position into which House Democrats were thrust after the 1980 elections. The Senate has never come close to being "fully decentralized." The treatment of Congress is very one-sided and lacks a sense of proportion; it certainly does not make the case that Congress is less capable of making good policy than it was 20 years ago.

Mezey is correct about Congress's parochial biases and is right to note that developments in interest group and electoral politics have pushed Congress further in that direction. But he offers little evidence to support the proposition that there is less consensus on policy questions now than there was 20 years ago. In fact, a plausible argument could be made that divisions on salient domestic and international problems are now less severe than they were in the late 1960s and early 1970s. Most importantly, none of the reforms that are proposed later in the book address the issue of how a democratic government of any form can meet Mezey's criteria of good policy (it must be informed, timely, coherent, effective, and responsive) if, as he insists, there is now little consensus about the definitions or solutions of important policy problems.

And that raises a vital point. As Cooper notes, policymaking is as much about choosing ends as it is about choosing means. A variety of conditions—degree of indifference, distribution of preferences—shape the conflict over ends. And the resolution of conflict over ends shapes the choice of means. Consequently, the nature of the political context must be central to an evaluation of the role of Congress. Unfortunately, Mezey's diagnosis is almost entirely dependent on his views about Congress's ability to engineer means; the problem of choosing ends—and choosing democratically with results that are considered legitimate under varying conditions—is not given due consideration. It is not sufficient to say that the president is more likely than Congress to have a national perspective and therefore should be granted the power to choose ends; for that assumes a great deal about how presidents actually get nominated and elected, about the nature of presidents' relevant constituencies, and about the character of the issues and the national interest, none of which receives rigorous treatment in this book.

In the end, this book leaves the reader with impassioned interpretations but weak supporting arguments. It provides no evidence that U.S. national policy is systematically poorer than policy developed in other systems, that national policy generally is now less adequate than in recent past decades, that the weaknesses of policy are caused primarily by the policymaking role of Congress, that the context that shapes Congress's current role will persist, or that the proposed reforms will significantly improve public policy.

STEVEN S. SMITH

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Without Consent: Mass-Elite Linkages in Presidential Politics. By Warren Miller. Lexington: University Press of Kentucky, 1988. 182p. $20.00.

Elite Cadres and Party Coalitions: Representing the Public in Party Politics. By Denise A. Baer and Michael Bositis. New York: Greenwood, 1988. 200p. $39.95.

Certainly a comprehension of the nature of linkages between the governing and the governed is fundamental to our understanding of the political process in a democratic society. Party elites, especially those serving as delegates to the national conventions, provide an essential component of this bond. Both books address the degree to which these elites represent the divergence of viewpoints on issues between elites and mass identifiers from both major parties. Each concentrates on the period between and including the 1980 and 1984 presidential elections. Baer and Bositis found, as did Miller, a complex movement of elites—and to a lesser degree, of masses—across a variety of issues between these two contests.

Baer and Bositis are the more ambitious in their work, thoroughly grounding their analysis of data from the Southern Illinois Party Elite Study on an underpinning of party, group, and social movement theory. Basically, this volume attributes much of the decline in U.S. political parties to a decline in party theory. Their notion that party theory has not adjusted to the changing nature of the parties brought on by the reforms of the seventies and eighties is persuasive. Parties have always been changing; party theory has remained in stagnation. They present evidence consistent with the "Mezzotropic representation model" predicting that the elites furthermost from the mass public should move closer to the center. The authors' conclusions are somewhat disappointing, however. Given the care with which the context for the analysis is laid out, this work cries out for much more discussion.

The Miller book is much more narrow in focus, reflecting its genesis as a lecture series. It is presented as a companion to Miller and Jennings's *Parties in Transition* and, as such, updates the study of convention delegates through the 1984 presidential election. Though in many ways this book could stand as a companion volume, it can well stand on its own.

Given its roots as a lecture series, *Without Consent* does not provide the reader with the background many might like or need. Miller is successful in adding to, and in many cases supplanting, much conventional wisdom in regard to apparent recent ideological shifts. For instance, he is persuasive in his argument that the Reagan Revolution "reflected a change in the Republican party elite rather than a mass movement" and that there was a slight shift to the left by both party elites and party supporters in the electorate between 1980 and 1984. His main finding is that delegates selected through primary elections are more representative of the mass base of the parties than those selected through party caucus.

Miller's book, while competent and insightful, might frustrate many readers in its closeness to the data, which come both from National Election Survey studies and from a mail survey of convention delegates. The treatment is rather informal, however, without hypothesis testing or much reference to what the reader should have expected based on a prior theoretical framework. Miller does present a gradual unraveling of intraparty and interparty incongruities and congruities, which are then discussed at some length in an almost conversational manner. His data analysis would benefit from a clearer presentation. For instance, it is never clear why he would use bivariate correlations as indicators of differences between two groups, as opposed to difference of means. While it is true that the measure does provide some standardization, that advantage is more than offset by the disadvantage of muddied interpretability.

On the other hand, while Baer and Bositis do base their analytic chapters on surveys of convention delegates and other party activists, the presentation of the data is almost anticlimactic, given the extremely well written and well conceived theoretical groundwork that had already been laid. For instance, the Baer and Bositis thesis—that not so much parties have been in decline as party theory—is quite convincing. In fact, Baer and Bositis provide the reader with so much insightful integration of party theory and institutional development that the volume would have stood without any analysis at all.

Both books make a real contribution to our understanding of modern political parties. Miller's work is especially useful when read following the more thorough treatment provided by Baer and Bositis. While the latter volume does seem to run out of gas at the end, Miller's ending essay is thought-provoking and lucid. Together, they do provide great insight into the dynamic nature of party politics and the failure of conventional wisdom to account for recent changes in a U.S. party system that is very much alive.

WILLIAM LYONS

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The Christian Right and Congress. By Matthew C. Moen. Tuscaloosa: University of Alabama Press, 1989. 234p. $32.95.

The swift rise of the new Religious Right caught many off guard, especially in the academy and elite press. Immediate responses ranged from hysterical fear to cavalier dismissal; the movement was variously depicted as an intolerant fundamentalist army and a figment of the media. Matthew Moen's new book provides a more measured and balanced analysis of both the strengths and limitations of the Christian Right, focusing particularly on agenda setting on the Hill. Based on extensive interviews of key actors and observation of congressional deliberations, Moen's work is one of the most detailed accounts we have of the interaction of Christian Right lobbyists and those they sought to influence. His thesis is that the Christian Right did influence the congressional agenda in the 1980s, a notable achievement for a movement so new to the Washington scene. Moreover, while actual policy victories were modest, Moen argues that by controlling the agenda, fundamentalists kept liberal forces on the defensive (for a time) and thus deflected initiatives hostile to their interests.

Moen begins with a discussion of the origins of the Christian Right movement. He nicely documents how diverse forces imploded on conservative Christians in the 1960s and 1970s. Viewing the world from their perspective, he shows how genuinely threatening were the libertarian excesses of the counterculture, the emergence of gay and women's liberation, the breakdown of the family, and the rise of an intrusive secular state. These trends, coupled with the rapid growth of evangelical churches, produced conditions ripe for political mobilization. Government agencies, notably the Internal Revenue Service and the Federal Communication Commission, then unwittingly provided the flash points that ignited the movement.

Translating concerns into operational political strategies proved daunting to Religious Right activists. Moen chronicles well the complex strategy sessions and negotiations involved in choosing issues and crafting tactics. One reason for the Christian Right's ability to shape the agenda was its concentration on a few issues of broad symbolic appeal to followers, a lesson some liberal church officials could heed. But if Moen illuminates this strength, he details as well the inner rivalries, diagreements over tactics, and competing policy remedies that plagued the movement.

Moen makes good use of the agenda-setting literature in framing his research, and he illuminates the roles of visible actors as well as behind-the-scenes players. One of the real strengths of the book is the way it traces in great detail the problem of "agenda specification," the process of crafting specific legislative vehicles to address broad policy concerns. In notable cases leaders of the Religious Right, along with their allies in the White House and on the Hill, failed to rally behind specific initiatives. There were strategic and philosophical disagreements over silent prayer versus recited prayer. On abortion, the dispute centered on the relative merits of "court stripping" proposals versus broad constitutional remedies. Tension within the ranks was never fully resolved.

In attempting to place his research into a broader theoretical context, Moen is less successful. Challenging explanations rooted in the "status politics" model, he argues that the Christian Right had decidedly practical and egalitarian objectives. But his depiction of fundamentalists as "egalitarian" leaves us unconvinced, centering as it does on their simple rhetorical demand for equal representation. It is probably more useful and accurate to describe them as populist.

In illuminating an important period in congressional history, *The Christian Right and Congress* ends on an unfinished note. Moen's research concentrated on the 97th and 98th Congresses; thus, his discussion of the second half of the decade is limited and incomplete. Moen speculates that the second Reagan term represented a "transition period" for the Christian Right, "from an offensive to a defensive strategy, and then in the wake of the television evangelist scandals, to an emphasis off Capitol Hill." Thus, he concludes that by the end of the decade the "prospects for the Christian Right in Congress were nonexistent" (p. 168). Moen does not provide much evidence for these sweeping assertions. Recent lobbying on the Civil Rights Restoration Act and the Act for Better Child Care tend to confirm the shift to a defensive mode. But Moen probably exaggerates the extent to which religious conservatives have retreated altogether from Capitol Hill. Even as Falwell was folding up shop, James Dobson of Focus on the Family was making his entry. What we need is a thorough update on the remnants of the Christian Right in Washington and a chronicle of their ongoing attempts to block or alter legislation they dislike.

Moen is doubtless correct in his assessment that many religious traditionalists seek to de-emphasize lobbying and concentrate instead on building institutions, such as secondary schools, universities, and think tanks, to influence the long-term direction of the culture. Unless the threat of modernity somehow fades, religious traditionalists will remain frustrated; building institutional strength is one way to capitalize on that angst for future political battles.

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Campaigns, Congress, and Courts: The Making of Federal Campaign Finance Law. By Robert E. Mutch. New York: Praeger, 1988. 217p. $42.95.

Twice this century there have been movements to reform the electoral process. Each movement has had two goals: reorganizing the political parties and regulating campaign finance. Robert Mutch's study of the passage, judicial interpretation, and administration of federal campaign finance laws is the first history of the struggle to attain one of those goals and its change over time.

The sources of campaign money have changed considerably in the last two hundred years. In the eighteenth century politics was a gentleman's pursuit, and candidates paid their own expenses. With the rise of political parties in the nineteenth century new sources of funding were found in regular assessments of the government employees dependent on the winning party for their jobs. This largely ended with the assassination of President Garfield by a disappointed officeseeker in 1881 and the subsequent creation of a nonpartisan civil service. Under the leadership of Mark Hanna, the dominant Republican party shifted to systematic solicitation of corporate money. The first federal campaign finance law was passed in 1907 to prohibit corporate contributions. The second was passed in 1911 requiring disclosure of contributions. Neither law was very effective, and in 1921 the Supreme Court ruled neither could apply to primary elections; Congress could not regulate the nominating process.

After the Teapot Dome scandal interest was renewed in regulating campaign finance. The Corrupt Practices Act of 1925 was the result. It didn't work much better, because there was no enforcement mechanism. Consequently, candidates and committees submitted financial reports in whatever manner pleased them. There was no review for accuracy or even a filing system. Not much happened for the next 50 years; but the rising cost of campaign spending —particularly for radio and television ads— was forging a consensus that more regulation was needed.

The Federal Election Campaign Act (FECA) passed in 1971 reflected that emerging consensus; but it was still so tentative that the act itself did little beyond strengthen the disclosure provisions and legitimize political action committees. This law was significantly amended in 1974 in response to a drastic increase in public awareness of the potential for corruption in campaign finance practices. The intervening variables were Watergate and the emergence of Common Cause, a "public interest" interest group with campaign finance reform as a primary goal.

The new FECA established strict limits on campaign contributions and expenditures and created the Federal Election Commission (FEC) to see that they were enforced. It was promptly challenged by a coalition of liberal and conservative groups that saw limits on campaign money to be limits on speech. Some limits on contributions had been in existence since 1911, but the lack of enforcement and the existence of loopholes had muted any strong opposition. The plaintiffs in *Buckley* v. *Valeo* were less interested in the actual effects of the new law than they were in the principle it represented.

The conflict between the principles of liberty and equality was resolved by the Supreme Court in 1976 by making several fine distinctions. First of all, it separated campaigns that received public funds (presidential campaigns) from those that didn't. The former could be regulated as a condition of receipt of public funds. It next established that the only constitutional basis for regulating nonrecipients was "corruption or the appearance of corruption" and concluded that this justified limits on contributions but not on expenditures. Thus, campaigns could spend as much as they had, and individuals and committees that were *independent* of a campaign could spend what they liked; but contributions to a campaign by these same individuals and committees could be limited by law. This decision set the stage for the next series of battles over public financing, negative campaigning, the role of political action committees, and the extent of enforcement by the FEC.

The struggle over "who should pay for our politics" has been a long and convoluted one, and little has been settled. There is neither agreement on basic principles nor on the consequences of different proposals. Partisan positions appear at one point in the debate, only to reverse themselves when the results turn out differently than expected. The "law of unintended consequences" has been the only reliable guide.

Mutch's study provides a great deal of valuable detail about this struggle but no framework for understanding it. Indeed, he often gets so mired down in the detail that he doesn't inform his reader which bill, law or court decision he is discussing at any given point. This book desperately needed a good editor to give it some coherence and remove such odd discrepancies as a two-page section on the Federal Election Commission that never mentions it (pp. 40–42) and an index that does not include the FECA. But for those already familiar with the field it offers alot of interesting research into committee hearings and congressional debates.

JO FREEMAN

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The Elections of 1988. Edited by Michael Nelson. Washington: Congressional Quarterly, 1989. 212p. $14.95 paper.

The Election of 1988: Reports and Interpretations. Edited by Gerald M. Pomper. Chatham, NJ: Chatham House, 1989. 228p. $25.00 cloth, $12.95 paper.

With the passage of every election season, the market is flooded with descriptive and analytic accounts of the most recently fought electoral battles. Two of the first edited volumes on the 1988 election, Michael Nelson's *Elections of 1988* and Gerald M. Pomper's *Election of 1988: Reports and Interpretations,* provide a readable and academically informative series of essays. Perhaps lacking some of the detailed analysis of later entries in the field, both volumes nevertheless offer relevant and timely discussions of the 1988 elections. Each provides students with a useful overview of the electoral process and scholars with the questions they will need to address in future research.

Both volumes follow a similar categorical framework: a general, theoretical overview, chapters on the nominations, the presidential election campaign, trends in public opinion, the role of the media, congressional elections, and a final interpretive summary. Although the books' separate authors employ several theoretical frameworks for their discussion—and these frameworks are shared across the two volumes—the analytic emphases of the two remain substantially distinct. For the most part, Nelson's contributors describe and analyze events within the context of historical, partisan, and institutional change, while Pomper's stress continuity with the recent past.

Francis Rourke and John Tierney set the stage for the analyses to follow in Nelson's volume. In making the assessment that the election of 1988 was (with apologies to Lloyd Bentsen) no 1960, they describe the changed partisan and institutional environment of electoral politics. No longer could the Democrats depend on the legacy of Franklin Roosevelt and the fear of harsh economic times under Republican administrations. Instead, the Democratic party was faced in 1988 with a withered New Deal coalition plagued by dissension over civil rights, Vietnam, social issues, the politics of interest group liberalism, and the changing image of the GOP as the party of prosperity. More importantly, no longer could the Democrats count on their superior grassroots party machinery to rally the troops. The campaign environment had changed drastically from "ground war to air war" (p. 12)—a careful, media-orchestrated assault on the public, beginning with a series of direct primaries, rather than a controlled, party-mediated final campaign by a selectively chosen candidate.

For the most part, the contributing authors hold to this framework.

Gerald Pomper prefaces his volume with the observation that the decision facing the electorate in 1988 was "whether to continue the existing political regime by electing George Bush as President or set off in a new, uncertain direction by the election of Michael Dukakis" (p. v). Walter Dean Burnham eloquently sets up the argument for the rest of the book. The election of 1988 and the political choices to follow need to be viewed within the context of the Reagan presidency and the realignment of style and substance that it promoted. Borrowing from Stephen Skowronek's (1984) framework linking presidential success with the temporal constraints of presidential action, Burnham describes Reagan as the most ideological president in recent history, "coming to power on the ruins of a repudiated regime" (p. 3) and acting out a revitalization of the political order. For the contributors to this volume, the actions and decisions of the candidates, the strategists, the media, and the voters were for the most part constrained by the political dialogue that emerged from Reagan's legacy of revitalization.

Several of the authors of both volumes attempt to answer (with varying degrees of precision and success) one common question: in Michael Nelson's words, "Did the 1988 campaign (and does modern political campaigning in general) fulfill even minimally the requirements of democratic government, especially the requirement that important issues of public policy be brought before the voters?" (p. ix). Paul Quirk assesses the campaign as one "so superficial and negative that it represented a perversion of the electoral process" (p. 63). He is followed in the Nelson volume by Thomas Patterson, who, by content-analyzing election news, determines that the media is ill suited to take on its new role as "principal intermediary between the presidential candidates and the voters" (p. 95). In his excellent analysis, Patterson places the blame for this sad state of affairs squarely on the demise of political parties and the direct public appeals that candidates must make through a laborious march through primary battlefields. Parties "have an incentive—the control of government—to organize in meaningful ways groups of voters that are demanding symbolic and policy representation in government. The press has no such incentive" (p. 96).

Others are less critical of the campaign and its results. Jean Bethke Elshtain disagrees with her cocontributors, arguing that the appeals of the 1988 candidates had real meaning and that analysts should "persist in presuming and evoking a cleavage between symbolic and real issues and themes" (pp. 112–13). Her dissent is reflected in varying degrees by Pomper's contributors. Rather than evading essential issues by focusing on the American Civil Liberties Union and the dreaded *L*-word, George Bush and the GOP were, according to Burnham, "operating in a realigned environment in which such strategy made rational sense" (pp. 5–6). Pomper states that in 1988—even if by accident—"the presidential nominations provided textbook examples of effective American political parties" (p. 33). Even if vague, "Bush's victory was more deeply rooted than an issue referendum. It was an assertion of basic American beliefs now more closely associated with the Republican party" (p. 145) and a demonstration of a long-term secular realignment toward a Republican presidential majority.

Several authors in both volumes specify limits on the referendum of the 1988 election and other recent elections. Electoral accountability, a necessary precondition of democratic governance, is somewhat muted by the duality of U.S. electoral politics, of a "split-level realignment" that makes it difficult, if not impossible, to attribute presidential and congressional outcomes to the same causes. It is, as Gary Jacobson states in his contribution for Nelson, "checks and balances with a vengeance" (p. 128), characterized by electoral majorities that prefer "a president committed to a set of diffused collective goods and congressional representatives who promise to minimize what voters have to give up to achieve them" (p. 145).

These editions are useful as sourcebooks for students and researchers not only because of their overview of the 1988 elections but because they continue a series of election volumes: Nelson's volume is the second in a series, Pomper's the fourth. I unabashedly admit relying heavily on Scott Keeter's analysis of public opinion for Pomper's 1984 volume and Farah and Klein's contribution in 1988 for my lectures on campaigns and elections in my introductory U.S. politics class. And I am always impressed with Jacobson's careful analysis of strategic politicians in his contributions for Nelson.

All in all, both volumes are successful in their use of different emphases in defining and analyzing the contours of U.S. electoral politics. As first academic entrants in the flood of electoral literature, these two volumes serve their readers well.

PETER F. GALDERISI

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City Games: The Evolution of the American Urban Society and the Rise of Sports. By Steven A. Riess. Urbana: University of Illinois Press, 1989. 332p. $29.95.

After many years of near-total neglect of the subject academics have discovered that organized sports play an interesting part in the social and political life of modern society and that it is possible to gain an enriched understanding of both sports and society by examining their interactions. Steven Riess, previously the author of an excellent study of baseball in the Progressive Era, has now produced an ambitious and largely successful effort to examine the connections between the growth and development of U.S. cities and the emergence of modern sports.

Riess selects several broad themes to organize his presentation. He begins with the accelerating process of urbanization itself in the mid-nineteenth century and traces the impact of growing population densities and changing work structures on urban space and recreational opportunities. He places appropriate stress on the development of a positive sports creed, manifested in physical culture programs, and the development of parks, all designed in some sense to provide a morally uplifting counterweight to the depressing effects of urban slum reality.

A second feature of urbanization was the differentiation into class and ethnocultural groupings, often territorially distinct and nearly always socially differentiated. The U.S. city circa 1900 was no longer an integral community but an unstable "ecology of games"; and Riess shows that the diverse class and ethnic subcultures had cultivated distinct preferences among different sports and had developed an extensive structure of voluntary sports organizations in which to pursue, quite separately, their respective tastes.

After the turn of the century, professionalization of sports made rapid strides toward its modern position of domination. Professional sports were and are business enterprises, of course; accordingly, one needs to look at the sources of capital and entrepreneurship and at the intertwining of politics and economics that undergird the sports page headlines. Riess shows quite effectively the extent to which professional sports franchises grew out of some of the more suspect shadows of urban politics and crime and the extent to which organized gambling has always been closely connected to organized sports.

The changing structure of urban life—from the walking city to mass transit to the automobile, to take one important dimension—is tied into sports history by virtue of its impact on the location and design of ball parks and domed stadia. Whereas the great appeal of a Wrigley Field or a Yankee Stadium was its ties to the mass transit system, a modern stadium must have massive parking and easy access to the expressways. The modern stadium, in addition, is almost always a public investment, built by the community to attract or keep professional sports franchise (plus rock concerts and tractor pulls) because of their economic multiplier effects on the local community. Riess gives attention to this aspect of modern urban sport but does not place it in context of development investment options in which a city might engage: sports palaces represent one way to attract economic activity to a community, symphony halls and art museums are another, housing for the poor is a third, and so on. Some are politically more feasible, some are financially more attractive, and some will yield larger net benefits. One doubts that in most communities these matters are ever weighed with the care they might deserve, but then that, too, is a deeply ingrained U.S. sporting tradition.

When an author has done so many things so well, it is ungenerous to complain. Nonetheless, I think that Riess gives too much attention to the Park movement and to the emergence of recreational values and concepts and too little to organized spectator sports. It is a matter of degree only, but I could have done with less on early bicycling and more discussion of the post-World War II decline of minor league sports and its impact on "minor league" cities. Surely, in recent decades the disjunction between most U.S. cities and prime-time professional sports has been a dominating feature of our lives, moderated, of course, by the explosive growth of professional athletics in division 1 universities. *That* shadow may be too close to home to permit the serious attention of political scientists for another generation or so, but the effects of spectatorism on U.S. communities and structures of loyalty and attachment warrant our careful examination.

ROBERT H. SALISBURY

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Post-Industrial Cities: Politics and Planning in New York, Paris, and London. By H. V. Savich. Princeton: Princeton University Press, 1988. 368p. $45.00.

New York, Paris, and London have experienced their decline as industrial centers and transition to postindustrial economies quite differently. H. V. Savich argues that governments have played a major part in creating those differences. Each city's response to industrial decline has been shaped by government decisions on public investments and land use regulation. Those decisions have been distinctive in each of these three great North Atlantic cities because of the different goals for public action and the different ways public choices are made in each place.

Savich presents three case studies in urban development for each of the three cities. His comparative analysis of these nine cases emphasizes the role of public officials in urban land use. Each of the three metropolitan areas has unique governmental arrangements; and the United States, France, and the United Kingdom have very different traditions and expectations regarding the role of government in directing markets. The cases are well detailed, the analysis cogent and insightful, the prose clear and colorful:

> New York's system contains a powerful tension between the objectives of planning and the profits of the marketplace. Developers are always on the prowl for opportunities, and if the planners do not come up with a scheme, sooner or later, private investors will.
>
> In New York the planner is not the master but the salesman. Like any salesperson, planners must use all the inducements they can muster. (p. 280)
>
> The centripetal pulls of the [Paris] system are formidable. Its resistance to premature intrusion of interest groups shields planners from what they regard as extraneous. The willingness to pour state and local resources into public projects means that the marketplace can be refashioned. The result is that planning has an altogether different meaning in France than in America. Planning is a long-range process whose objective is to reconcile the parts to the whole. (p. 281)
>
> London's liberal corporatism is drawn from an altogether different history. Its politics has been caught in an ambivalence between a respect for local democracy and a commitment to central authority. London has also inherited an ambivalence toward capitalism, drawn on the one hand from the naked industrial competition of Dickens' London and, on the other, from the moderating influence of Fabian socialism. (p. 301)

Savich sees politics as the interplay of officials and citizens, each of whom may pursue various public and private interests. He adroitly demonstrates how the allocation of authority for decisionmaking shapes and channels politics (and how politics can, in turn, restructure governmental arrangements, as in the abolition of the Greater London Council). His analysis moves deftly back and forth between the human scale of action and choice and the metropolitan scale of urban development patterns.

This book will be of interest to several groups of scholars. Urbanists may be especially interested in Savich's insights into the politics of economic development. (He argues against the thesis of a general probusiness bias.) Political economists may find his comparison of local land market management in the three cities of particular note. The "new institutionalists" will find his (nonformal) analysis of how governmental arrangements structure political behavior useful. In short, this book shows us how to think more clearly about politics in general, as well as telling us about a particular set of policies in three distinctive political communities.

GORDON P. WHITAKER

University of North Carolina, Chapel Hill

Interactive Corporate Compliance. By Jay Sigler and Joseph Murphy. Westport, CT: Greenwood Press, 1988. 211p. $39.95.

This book promotes the development, in the U.S. corporate world, of stronger corporate compliance programs as a means of self-regulation to overcome the limitations of a society increasingly paying the costs for the existence of adversarial relations between government and business (see Alfred Marcus's *Adversary Economy* [1984]). Murray Weidenbaum has argued that in the modern corporation there exists a government regulatory agency that oversees each of the classic business functions. The first managerial revolution brought about the separation of ownership and management in the modern corporation. The second has meant, according to Weidenbaum, the taking away of traditional managerial functions by government bureaucrats and planners. A more positive interpretation of this development has been provided by Lee Preston and James Post in their book *Private Management and Public Policy* (1975), in which they argue that a principle of public responsibility—or responsibility to the public policy process—has come to replace more traditional notions of social responsibility in the modern corporation. Sigler and Murphy interestingly develop some of the implications of this change from social to public responsibility—or, in Weidenbaum's terminology, from the first managerial revolution to the second.

Lawyers, Sigler and Murphy claim, are trained to maintain the adversarial quality of the present system. They are not trained to obtain consensual compliance from corporations. As a consequence, public policy outcomes are frustrated in the implementation stage by an overly litigious system. In place of this, Sigler and Murphy propose to use corporate legal counsels as compliance officers intended to get the firm to carry out public policy in a cost-effective fashion. Perhaps the most important chapter in the book is the seventh, where the authors discuss what it would take (not surprisingly) to achieve corporate compliance: (1) management commitment, (2) education and awareness, and (3) implementation and control. The comprehensive program for corporate compliance that they outline would make the corporation's sensitivity to the law and to legal requirements far greater than it presently is. They recognize some of the practical problems that would necessarily affect such a comprehensive program; but whether corporations can be expected to make such drastic changes in how they operate is uncertain.

An important contribution is the discussion of the constraints that legal disclosure laws place on corporations. To run a good compliance program the corporation will have to undertake many internal audits to ensure that programs are being carried out within the confines of existing law. Any time these audits reveal deficiencies, they place the corporation at legal risk. If a paper trail exists showing these deficiencies, it will be seized on by plaintiff attorneys and used against the corporation to win large punitive damage awards. Now, there is little corporations can do to get around this dilemma other than to invoke lawyer-client privilege and have corporate auditors and evaluators write uncritical documents. The current environment does not seem to be very encouraging for the development of the type of interactive compliance programs that Sigler and Murphy advocate.

A second interesting argument is that governments have to do more to promulgate the law adequately if they expect better compliance. A corporation—or for that matter a person—can be held liable for violating a law only if the intent of the law and what is required for compliance are properly understood. Regulatory agencies are often unclear about what they want, and the only way to achieve greater clarity is through test cases. Sigler and Murphy lament this process.

This book is less a scholarly tract than a practical hands-on book that advocates a particular position and explains how to implement it. In the genre of advocacy books, the Sigler and Murphy volume has obvious merits but also some demerits. I would have liked a better explanation of how the chapters fit together. Often I felt as if I were reading a series of separate memorandums on the issue. Another problem was that the scholarly chapters on the history and development of regulation were not as strong as the chapters in which Sigler and Murphy actually discussed corporate compliance programs and the need for them. Still, the authors have explored an important issue in unusually great depth and placed their insights in front of us to debate. This book might be usefully read both by scholars of business-government relations and by policymakers and attorneys who are involved in the compliance process.

ALFRED A. MARCUS

University of Minnesota

The Transformation of the U.S. Senate. By Barbara Sinclair. Baltimore: Johns Hopkins University Press, 1989. 233p. $28.50.

Call to Order: Floor Politics in the House and Senate. By Steven S. Smith. Washington: Brookings Institution, 1989. 269p. $31.95 cloth, $11.95 paper.

Don't trust any book over thirty! In 1990, Donald R. Matthews's justly famous *U.S. Senators and Their World* will have passed this milestone. The paperback version is now out of print; and, classic though it is, even its author (in a 1988 APSA convention paper) admits that the Senate of the 1980s is very different from the one he described of the 1950s. Now, just in the nick of time, as Matthews's book goes "over the hill," come two new books, one describing the Senate, the other the Senate and House of the 1970s and 1980s. These are the first two book-length studies of the entire Senate (and House); and both clearly indicate that the "textbook" Congress we children of the 1960s were weaned on is as distant a memory as the Vietnam War protests that undermined much of the comity in Congress and led to the new era.

Not surprisingly, though, what goes around comes around. The "new institutionalism" of the 1970s and 1980s has to a considerable extent been built on stylized models that incorporate much of the old "textbook" wisdom: strong committees can obtain their way on the floor and then ensure their victory through their control of conference committees. These "little legislatures" are especially privileged by the rules of procedure, especially in the House. That body, with its more precise formal rules, is more susceptible to equilibrium results than the always-disorderly Senate.

Sinclair and Smith present a very different picture. They show us a Senate (and a House) in which committee autonomy has frayed over the past two decades. More members not on originating committees offer amendments to legislation (Smith), more of these amendments pass (Sinclair, Smith), the committees themselves are more representative of the full chambers than previously (Sinclair) and thus are less in need of autonomy, and the Senate's contested workload has increased dramatically (Sinclair), as has the sheer volume of activity in both houses (Sinclair, Smith). The Congress is now much less a rule-bound institution. Norms have frayed, especially those of apprenticeship and reciprocity, and the locus of action has shifted from committees to the floor.

These two books make the same general argument using similar data bases. While Sinclair also employs interview data with high-level Senate staff members, both she and Smith primarily rely on internal congressional workload measures. Both have done a monumental job of gathering these data, Sinclair for every other Senate from the 84th to the 99th Congresses (1955/56–85/86) and Smith for selected Houses and Senates for the same time period. They also share the same general perspective. The major determinant of the explosion of floor activity and the waning of committee autonomy was change in external environment: new issues and new groups arose, the agenda became more crowded and antagonistic, and the "iron triangles" of the 1960s and before simply disappeared into an ocean of contentiousness.

There are, of course, differences other than the most straightforward one of focus (Sinclair exclusively on the Senate, Smith on both chambers). Sinclair's book is a clear successor to *U.S. Senators and Their World.* It gives us a collective portrait of the Senate and focuses on changes in ideology and styles as much as on aggregate measures. Indeed, two chapters contain no hard data at all. Smith's book is more specifically institution-oriented. The theoretical focus should be familiar to those who have followed his work: the shift from decentralization in the Congress to collegial decisionmaking. References to the external environment are just that—passing shots not developed in any great detail. However, there is a richness of detail about House and Senate procedures and an explicitly comparative focus that more than compensates for this weakness.

Smith insists that rules make a difference, and his data support that conclusion. Yet, like

Sinclair, he ultimately comes down against the "new institutionalism" (quite correctly) for failing to emphasize the endogenous nature of rules. In other words, rules are strategies. Their success is highly contingent on the distribution of attitudes toward specific policies. Committees are not autonomous now, and it is unlikely that they have ever been so. Additionally, Sinclair's external focus leads her to place a heavy emphasis on the role of the media, especially television, something that Smith downplays considerably. I generally agree with Smith but was impressed with Sinclair's data analysis showing that measures of activism had an effect independent of committee positions on television and print coverage of senators. If she had convinced me that collinearity was no problem, I might have had to rethink some of my own positions.

Of course, I have quibbles large and small with each book. No book that really stimulates can fail to provoke a good argument, and both Sinclair and Smith have produced extremely important works. Each author uses the prisoners' dilemma (PD) to discuss norm abidance, yet both drop the PD game once the discussion of norms is complete. (Sinclair, to be fair, does briefly employ the analogy at the end of her book.) Sinclair argues that adherence to norms was "less expensive" for individual members in the 1950s than it is in the contemporary Senate. I believe that she is correct; but if so, the applicability of the PD game to norm abidance in the earlier era is suspect. As Michael Taylor argued in *The Possibility of Cooperation* (1987, p. 29), if people obey a norm because it pays to obey, there is no collective action problem, hence, no PD. Neither Sinclair nor Smith offers us a resolution to the collective action problem, although Smith comes much closer, arguing that it was largely reputation rather than sanctions that underlay the norms of the 1950s.

Both discuss environmental factors—Smith briefly but Sinclair at greater length (all of chapter 4)—yet in each case the discussion of the environment appears and disappears like a jack-in-the-box. Neither author relates such external changes to the larger, macro political economy. Smith's "collegialism" model is a mark of clear thinking on how legislatures are organized, but it too vanishes after chapter 1 and does not explicitly come back until the conclusion. And I for one, wish he would make up his mind as to where he stands with respect to rules as institutions. Often, he seems to embrace the "new institutionalism" (especially with respect to the effects of electronic voting) only to come down as a critic of it. Smith then proposes his own structural reforms (modest though they be) but leaves us with the faint feeling that he doesn't have much faith that they will help restore order to the Congress.

These are both books that will provoke many good arguments. Each could have been more clearly developed in terms of a consistent theoretical focus that explicitly guided every chapter; as it is, both sometimes get bogged down in data, the effects of (sometimes arcane) rules changes, and comparisons with earlier Congresses. However, both make a major contribution and will set the agenda for much of what congressional scholars do for the next thirty years. Each offers a wealth of information to which I cannot do justice here. Sinclair considers changes in the committee assignment process, the role of issues in changing the Senate's agenda, the determinants of floor activism, and the (limited) role of membership change on the emergence of a "new Senate." Smith examines the determinants of activism, pays close attention to the import of formal rules in the House, and presents a brilliant analysis of the limited role of conference committee's ex post veto powers. Any serious student of Congress who has failed to read both books by the time this review appears will be remiss in professional duties.

ERIC M. USLANER

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Getting into the Game: The Pre-Presidential Rhetoric of Ronald Reagan. By Mary E. Stuckey. New York: Praeger, 1989. 216p. $37.95.

Stuckey has studied Ronald Reagan's speeches, articles, and radio broadcasts in three phases of his political career. She calls our attention to adjustments of text and tactic; for example, after Nixon became president, the loyal Republican governor of California muted his anticommunism theme, shifted the brunt of his antigovernment attacks to Congress and the bureaucracy, and burnished his public identity as a good citizen reluctantly drawn into politics by his sense of duty to the values his country represented. Stuckey's main contention is that "political rhetoric becomes less substantive as it becomes more national" (p. 2), a dangerous transition that Ronald Reagan accomplished in a unique way.

Stuckey's analytic framework is narrow, and her writing style fragmented. Since she has excluded from consideration the rhetoric of other politicians, private and official communications by Reagan, and press and scholastic accounts of the events Reagan invoked to make his points, readers have no way of validating her claims about how typical or moral Reagan's rhetoric was. Worse, her organization of these claims turns *Getting into the Game* into a jigsaw puzzle. The eighty pages of text contain political overviews for each phase (1960–67, 1968–74, 1975–80), followed by generalizations about Reagan's persuasive techniques and commentary on three exemplary speeches. The commentary shows insight. But such remarks as "In paragraph 25, Reagan once again establishes his identification and empathy with the audience while establishing the alienation of the government from the people" (p. 23) make little sense when the paragraphs themselves have been banished to an appendix. Another appendix outlines the rhetorical terminology but lacks a model of oratory. Remarks about trends and patterns in Reagan's rhetoric appear in the footnotes.

What could have been a significant monograph is, unfortunately, a repetitive and largely unedifying assemblage. However, like the silent dog in the Sherlock Holmes story, the evidence contained in this book offers a clue in reverse to Reagan's rhetorical success. Broken down into mechanical parts, the Great Communicator's attempts at persuasion seem quite ordinary. He blamed general ailments on the other party's leaders instead of identifying systematic flaws. He appealed to patriotism and relied on homiletic anecdotes. And, as Stuckey argues, he gradually confined his crusader talk and imagery to audiences of the converted, devoting more time to topical tidbits in which his issue positions were sandwiched between "extreme" alternatives, so as to make them look sensibly moderate.

But other national politicians of the era used these techniques, most similarly the early Jimmy Carter, Jesse Jackson, and Pat Robertson. Perhaps, then, the communicative magic that helped to lift Ronald Reagan above rivals, detractors, and would-be successors lay beyond his words. As Kathleen Hall Jamieson (*Eloquence in an Electronic Age*) and Garry Wills (*Reagan's America*) have shown, Reagan commanded a panoply of telegenic tics and gestures that familiarized his reactionary messages; and his entertainment career projected him as a personification of the same idyllic "America" his rhetoric advocated in policy goals. He was our political Walt Disney, the man whose name millions instantly associated with pop-up myths.

Stuckey acknowledges the importance of Reagan's image in a footnote to her conclusion, which attacks the hollowness and ephemerality of his political discourse (p. 81). Her deconstruction of Reagan's verbal appeals to general audiences provides little justification for her critique.

MICHAEL CORNFIELD

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Wildlife and the Public Interest: Nonprofit Organizations and Federal Wildlife Policy. By James A. Tober. New York: Praeger, 1989. 220p. $39.95.

This book makes a solid contribution to the rapidly growing literature on environmental policy and politics. In particular, the author effectively reviews the involvement of conservation organizations in the formation of U.S. federal wildlife policy, especially in the 1970s and 1980s. He does this at a level of generality sufficient to allow readers to learn much about the structure of the U.S. environmental movement as a whole and something of its history in this period. The book is particularly useful in its presentation of data on organizational membership patterns, fund-raising methods, and policy input techniques.

The overall focus of the book is on (in Tober's curious phrase) the wildlife "industry." The range and scale of organizations involved in wildlife policy formation will be surprising to most readers. Tober concentrates his attention on the role of nonprofit organizations in this process; and even here there is a considerable organizational variety in terms of objectives, styles, political perspective, and scale. In concentrating his attention on nonprofit organizations Tober makes a contribution to the general study of the political and social role of such organizations. But there is, perhaps, some loss here in terms of the analysis of wildlife policy formation; that is, such policies are highly susceptible, of course, to the influence of private, for-profit, corporations and government agencies. Tober is very mindful of their role but at times seems not to give it due emphasis.

Within this book are imbedded case studies of policy formation regarding the bobcat and the California condor. Here, Tober can get down to the specifics of what wildlife NGO's actually do to influence the policy process. The studies were well chosen, and the cases are nicely delineated in a way that illustrates how decidedly nonmonolithic wildlife organizations are as a collectivity. This finding is important to the specific study at hand and may have some more general applicability in relation to studies of interest groups operating in other policy arenas.

One shortcoming in this study as a whole is, I regret to say, widespread in our discipline and in the contemporary social sciences generally: it tends to be ahistorical. Tober, to his credit, opens with a few interesting paragraphs regarding historical context but within two pages has taken his readers into the 1970s. Now and again he touches on the earlier period but rarely for long. It is too easy to fault an author for not doing everything, but this study in particular would have gained considerably from a chapter developing the history not only of the policy issues and organizations involved but, in particular, of the policy context: something of the history of wildlife and wildlife habitat in the United States and elsewhere. Only within that context can nonexpert readers get a meaningful sense of what realities have spawned the considerable level of public concern and enormous organizational capacity that Tober so effectively delineates.

Tober's analysis of the participation of nonprofit organizations in the policy process provides the core for his concluding chapters. The analysis is clear and intelligent and should be useful to those who study public participation processes more generally. Tober develops a typology of five distinct perspectives on the involvement of the public and NGOs in the policy formation process. These perspectives include, for example, a view that participation "has been extended too far, forcing agencies into incorrect and hastily conceived decisions under pressure from vigilant constituencies" (p. 180). Also included are views that see policy as essentially technical, that see participation as useful and effective, and that see participation as the best means of achieving synthesis, as opposed to "mere" analysis. This typology could, perhaps, be refined further. But it is, in my view, potentially useful within the discipline as a whole.

Some analysts, myself included, have asserted that environmental politics is a distinctive politics in several ways. Tober does not explicitly take such a view here. Nonetheless, his detailed assessment of environmental organizations and their behavior is important to any appreciation of the evolution of environmental politics in the contemporary era. One hopes that he or others will follow with similar studies of organizational behavior in issue areas across the range of environmental concern.

ROBERT PAEHLKE

Trent University

Black Presidential Politics in America: A Strategic Approach. By Ronald W. Walters. Albany: State University of New York Press, 1988. 255p. $54.50 cloth, $19.95 paper.

How can blacks best achieve political power? What strategies hold the greatest promise for realizing policy ends of direct benefit to the black community? These are the problems Ronald W. Walters, a political activist and an academician—a "committed scholar" in his own terms (p. xii)—explores.

The assumption is that African-Americans, despite increased political activity and a series of victories in big city mayors' races, have yet to achieve an equitable share of public goods or to exercise influence proportionate to their electoral base. The argument is that black needs have not been well serviced either in the Reagan years (a position not difficult to document) or through intraparty bargaining strategies within the Democratic party.

Walters examines in-depth the "balance of power" arguments emphasizing the centrality of black support for candidates and political parties. He weighs the contentions in favor of a "dependent," or intraparty (realistically, intra-Democratic party) approach and "independent" (either between parties or in support of the creation of a black political party) strategy.

The merits of each alternative have been debated by activists, practitioners, and academicians for over a century. Walters's position is to challenge the results of working within a single party, although he does not totally reject such an approach. Rather, he advocates that African-Americans strengthen their political base to enable them to bargain effectively with any political party:

> Blacks should consider achieving greater organization of their political resources (votes, funds, mobilizational skills, issues, etc.) in an effort to acquire the flexibility and strength which will increase their competitive position. This view challenges previous assumptions of political behavior by recognizing the arena within the coalition to be a legitimate theater for implementation of the "balance of power" concept. . . . The basis of organization within the party is challenged, since Blacks are encouraged to complement political integration strategies with autonomous organization. . . . Autonomous political organization provides the flexibility to select voting or non-voting options either inside or outside of the party coalition. (p. xiv)

Jesse Jackson's presidential drives illustrate the potential for developing black political resources, increasing the leverage of African-Americans, and providing a base that future political operations can exploit.

The argument is subtle. Walters is not saying that a black has prospects in the near future of winning the presidency or that Jesse Jackson as a leader or a Jackson-like campaign are the only viable options. This is not his intent. The book ranges well beyond what is essentially a modest, but insightful treatment of Jackson's presidential bid to assess more inclusively the principal alternatives available to African-Americans. Although not an overly lengthy analysis, it covers a good deal of ground.

There are some problems. The argument is not always easy to follow. Greater simplicity and clarity of position conceivably could have added to the book's impact.

The author reviews, by my count, four basic strategies. These are (in my terms) the within-party incrementalist strategy (favored by black elected officials); the within-party assertive strategy (the Jackson approach); the between-parties bargaining strategy (not in contemporary use); and an independent black party movement (not in contemporary use). Basically, Walters favors the latter three (with an argument to give renewed consideration to the last two) and chooses to emphasize a strengthening of the black's organizational and electoral base. This is reasonable but broad and, I expect, difficult to implement. Many ambitious efforts along these lines have had limited payoffs.

At a different level some of the interpretations are highly judgmental. This criticism is not compelling in relation to the major theses. At times, however, the assessments can be central to the immediate arguments. For example, in looking at black Democratic and Republican 1980 national convention delegates, Walters makes the point that the positions that separate the delegates by party are less significant than the community of interests held in common. This may be so. But from the data provided (Table 8.1, p. 187), I was considerably more impressed by what divided black Republican and Democratic national convention delegates than by what unified them. Significant differences occurred in their stands on tax cuts, health insurance, welfare reform, and military policy. Further, demographic information on the economic and class structure of African-Americans in the two party groups was not included. The presumption is that there are notable contrasts. This is, of course, but one aspect of a broadly textured analysis; and it—and other interpretations that might be open to challenge—may be the result of differing perspectives.

The book has many strengths. The development is knowledgeable and the treatment evenhanded. The ability to combine practical political experience and a firsthand knowledge of events with a political science orientation is impressive. In this vein alone, the book supplies a useful overview of recent events relevant to an appreciation of black presidential politics. The scope of the effort is broad in design and the historical analysis of previous controversies and movements is helpful in placing present efforts in an understandable context.

African-American politics and its implications for democratic practice are under-explored by political scientists. For reasons that are unclear, such movements receive less than their due. Such books as this one help to redress the balance by focusing on issues often not given the attention they should receive.

Finally, and most importantly, there is the centrality of the questions raised: Are blacks poorly represented by the system? If so, how can African-Americans best achieve their political objectives? Walters's analysis provides the grounding for such a debate. It is a major contribution to understanding the issues in contention and the alternatives open to black Americans.

WILLIAM CROTTY

Northwestern University

Presidential Influence and the Administrative State. By Richard W. Waterman. Knoxville: University of Tennessee Press, 1989. 257p. $37.95 cloth, $18.95 paper.

In *The Administrative Presidency* (1983) Richard P. Nathan described and endorsed presidential use of a multiple administrative strategy to gain control of the bureaucracy. Waterman, sharing Nathan's belief in a strong presidency, reappraises the administrative strategy with, he says, the "intent not to refute Nathan's approach to the expansion of presidential influence, but rather to refine it" (p. 10). This understates the line of argument. Waterman finds that "the risks of the administrative presidency strategy have often outweighed the benefits" (p. 12) and urges presidents to adopt a cooperative style rather than the confrontational style he finds advocated by Nathan. In the book's final sentence the author rather startlingly says that "presidents must realize that the bureaucracy is their ally" and, following Neustadt, concludes that "the bureaucracy will be a better ally to one who leads by persuasion than to one who leads by command" (p. 193).

The heart of this well-documented book is the recounting and analysis of four cases of presidential use of administrative strategies: Nixon and the Department of Housing and Urban Development, Carter and the Interstate Commerce Commission, Reagan and the Environmental Protection Agency, and Reagan and the Nuclear Regulatory Commission. The tools used by presidents in these cases were variously the powers of appointment, removal, and reorganization; budgetary authority; transfer of program authority to the states; central clearance; and cost-benefit analysis.

In both general chapters and the case studies the author traverses familiar ground. The former review the literature on the role of the president in a shared-powers government, the justifications for his influence over the administrative state, and his tools for exerting that influence. These chapters are competent but break no new ground. Similarly, the case studies fall in fields much explored by the policy analysis community and are unlikely to strike members of that community as novel.

Distinctive is the focus on presidents' choices among administrative tools of influence together with appraisal of those tools' utility. The lessons drawn are sensible but not surprising. For instance, that the appointment power is the president's most potent weapon but that managerial ability and Washington experience should rank with loyalty as criteria; that a president's budgetary requests gain legitimacy if his appointees support the budget before Congress; that short-term benefits may be undone by adverse long-term consequences; and that resort to administrative tools is not risk-free, for it may excite confrontation by Congress, interest groups, and the bureaucracy.

JAMES W. FESLER

Yale University

Bureaucracy: What Government Agencies Do and Why They Do It. By James Q. Wilson. New York: Basic Books, 1989. 433p. $24.95.

James Q. Wilson has written, crisply and clearly, a thoughtful and often insightful book on U.S. public bureaucracies. He states that he does not intend to make the book primarily theoretical or primarily practical. He steers between these options by developing, and then musing on, numerous examples from the real world that illustrate a wealth of general observations.

Although he opines, "I have come to have grave doubts that anything worth calling 'organization theory' will ever exist" (p. xi), he offers a rich array of generalizations that are, in principle, testable and are of theoretical interest, even if they are not part of a fully developed comprehensive theory of organizations in general or of public bureaucracies. For example, "Government agencies are not billiard balls driven hither and yon by the impact of forces and interests. When bureaucrats are free to choose a course of action their choices will reflect the full array of incentives operating on them: some will reflect the need to manage a workload; others will reflect the expectations of workplace peers and professional colleagues elsewhere; still others may reflect their own convictions. And some will reflect the needs of clients" (p. 88). That level of theoretical importance satisfies me, though others may wish for more.

The essence of the book is descriptive, "an effort to depict the essential features of bureaucratic life in the government agencies of the United States" (p. 27). The depiction relies on examples spelled out in clear English, not on quantitative data or either statistical or formal models.

The chapters are discursive, but most of them have succinct summaries that make clear Wilson's views of the general lessons supported by the examples he has adduced. The examples come from his own observations, which are extensive, and from his detailed knowledge of relevant literature.

The topics flow nicely in a logical order. In what I think is the best part of the book (a roughly-two-hundred-page section ending on p. 378) Wilson distinguishes in detail among the behaviors of three different kinds of bureaucrats: *operators*, *managers*, and *executives*. He skillfully demonstrates how these different layers of employees in public bureaucracies have different mixes of incentives, goals, constraints, and opportunities and why their behavior varies among types as well as among individuals within each type. He also explores institutional constraints on U.S. bureaucracies, some differences between our public bureaucracies and those of other nations, the nature of common complaints about bureaucracy in the United States (especially inefficiency and arbitrariness), and two major ways for dealing with the alleged problems: *rules* and *markets*.

My only complaint about the book—and a mild one it is—is that it lacks an annotated bibliography. A pithy guide to the literature from Wilson's perspective would have been a valuable addition.

Particular values about social science might well make particular readers wish Wilson had done something fundamentally different. If other individual scholars want to do something different either theoretically or empirically, that is fine; we can compare their work with his. But it is important to underscore here that what Wilson chose to do, he did very well. Everyone interested in bureaucracy, regardless of his or her notion of how political scientists should analyze it, should read and profit from this book. Few books in our discipline deserve that kind of accolade.

RANDALL B. RIPLEY

Ohio State University

COMPARATIVE POLITICS

Restructuring the French Economy: Government and the Rise of Market Competition since World War II. By William James Adams. Washington: Brookings Institution, 1989. 400p. $36.95.

Students of comparative politics have long been intrigued by the respectable performance of the postwar French economy. As is pointed out by William James Adams, the French economy quadrupled in size between 1950 and 1985; and per capita income in France, compared to that in Britain, surged from a 20% disadvantage to a 20% advantage. Yet, particularly under the Fourth Republic, the French presumably were handicapped by constitutional instability, frequent ministerial crises, and a fragmented and polarized party system. What accounts, then, for the French economic miracle?

According to the conventional wisdom, the French political class after the liberation was determined to modernize the economy and society yet had no confidence in the ability of the capitalist class to do the job. French enterprise tended to be small, family-owned, starved for capital, and technologically backward. So the regime created a surrogate for the capitalist class—a corps of brilliant young civil servants recruited and trained by the state itself for the express purpose of shattering traditional economic structures and creating a modern economy. The administrative elite was a combination of battering ram (splitting apart the old economy) and magic wand (calling into existence the new).

Adams provides a useful corrective (despite statistical overkill and an awkward writing style) to the prevailing view. He concedes that government intervened actively in the economy after the liberation, playing a considerable role in directing immediate postwar reconstruction. But, he contends, government initiatives cannot account for structural change that took place throughout the economy. More important was the rising exposure of French firms to genuine competition, brought about by the creation of the European Community and deregulation. In calling attention to the limits of the almighty administrative state and the importance of cultural changes within the managerial class, Adams reflects the current fascination of French economists with the market.

In terms of the author's own argument, however, the key developments took place only in 1959 (Treaty of Rome, requiring another decade to become effective) and 1978 (when deregulation began in earnest under Raymond Barre). Yet the period of rapid economic growth dates from around 1948. It is more plausible to postulate a state-driven model as dominant from the liberation until Pompidou's presidency, when dissatisfaction started to build up concerning state initiatives and technocracy.

At the very end of the book the author addresses an exciting issue, now at the center of debate among French social scientists: how to position the economy for the next great challenge. Gradually replacing the old mass production industries (with their giant factories and sullen workers) is a decentralized network of small enterprises able to modify methods frequently in order to achieve product differentiation. Adams leaves us with the tantalizing thought that the decisive factor in global competition will be the "quality and flexibility of the labor force." Yet he makes only an oblique reference to the Auroux laws (not even naming them), which established a new structure of dialogue between labor and management, paving the way for a remarkable expansion of the quality circle movement.

Business in France is now undergoing a mutation. The old patriarch is becoming extinct, and graduates of the *grandes écoles* no longer reign supreme. Enterprises at the cutting edge of development are increasingly characterized by a constant flow of information among all participants in the productive process. Trade unions have been caught short by this development and are desperately trying to adapt. The substructure of French politics is undergoing profound changes as labor and management, along with parties of the Left and the Right, devise new approaches and policies. Here is the next item on the research agenda of students of French politics.

BERNARD E. BROWN

City University of New York

Security and Economy in the Third World. By Nicole Ball. Princeton: Princeton University Press, 1988. 432p. $39.50 cloth, $16.50 paper.

For a number of years developmental economists, political scientists, and government officials in a wide range of countries have debated a complex question: What is the relationship between defense spending on the one hand and political and economic development on the other in the "less-developing," or poorer, countries of the world? This volume (which uses the euphemism Third World) nicely summarizes that debate in balanced fashion; it is unlikely to be surpassed in the near future. Its merits stem from the unorthodox nature of the guns-versus-development controversy, its few defects also flow from the author's reluctance to move much beyond the terms of that debate.

Nicole Ball (who is now with the National Security Archives) has, over the years, been something of a one-person think tank on developmental and defense issues. Absent a large-scale, coordinated, interdisciplinary study by scholars with firsthand knowledge of a wide range of countries, she has managed better than anyone else to combine generalization and case studies.

Part 1 explores, in sufficient detail, the longstanding methodological issues: How does one measure defense expenditure across national and cultural boundaries? There are several useful cautions for anyone who expects to use SIPRI, ACDA, or (least reliable, according to Ball) IISS data. How can one reconcile the several theories that predict the likely impact of military spending on development? What does one make of the use of aggregate data (abuse, as Ball takes on the important early study by Emile Benoit, which detected a favorable association between defense spending and development in middle-level countries and has been used to justify high levels of defense spending in a number of large states, including India).

Part 2 explores the linkage between security expenditure and different kinds of development. What is development? Does one measure overall growth rates, the development of expertise and skills, or progress in a particular sector (e.g., electronics or heavy industry)? A series of appendixes explores several definitions of military expenditure and the nature of defense budgets in the Third World, compares the several empirical studies of the growth-military relationship, and provides data on the percentage of "economically active" in the security forces.

It is hard to see how anyone will write about the role of the armed forces or the expansion of advanced weapons into the non-Western regions of the world without frequent reference to this book. But some cautions are in order. These include a tendency to unsubstantiated generalizations about the nature of security threats to Third World states. The assertion that "external security threats facing most Third World countries are minimal" (p. 34) reflects the authors' perceptions, not those of a good number of states outside of Europe or students of the Middle East, South Asia, or Central America. More fundamentally, Ball ignores the fact that for many politicians in many states, security is a value in itself and in some circumstances may, indeed, be the prerequisite for growth. A lot of humbug and fraud accompanies the defense budgets of dozens of states, but these can be found in the West and East, as well as the developing regions. Finally, chapter 8 on the relationship of military manpower and human capital formation is frustratingly inconclusive. There are bits and pieces of studies from a number of countries, but the detailed, comparative data base does not exist and may be impossible to contrive.

There are other flaws, of course, but none are fatal. Ball deals with an important, real-world problem. We are in her debt for persisting over a number of years, bringing her past work and thought, and the work of a number of others, together into this fine volume.

STEPHEN P. COHEN

University of Illinois, Urbana

Elections and Democracy in Central America. Edited by John A. Booth and Mitchell A. Seligson. Chapel Hill: University of North Carolina Press, 1989. 214p. $29.95 cloth, $10.95 paper.

This important book works well at many levels: as a theoretical discussion (utilizing recent literature) on the nature of democracy; as a useful chronology of elections in Central America, especially since the mid-1970s; as a vigorous debate over the extent to which the Central American nations have become more democratic in the 1980s; and as a major contribution to the sharpening arguments over whether United States policy in the region helped or hindered democratic movements. The last point is pivotal. In his succinct conclusion, John A. Peeler argues that the Reagan administration cannot take credit "for the emergence of democracy" in Central and South America. The United States "decisively shaped" Central America, "but the regimes established and the elections held under strong U.S. pressure cannot by any reasonable standard be called democratic, and their prospects for long-term stability are minimal" (p. 185).

John A. Booth provides a theoretical framework for this and other key points by broadly defining democracy as "popular participation in rule" (p. 13) and then listing criteria by which not only popular participation but the nature of the rule can be measured. By dealing with the criteria to a greater or lesser extent, the essays develop common themes, although not common conclusions. Booth notes that the essays are not concerned with the "narrower, electoral focus" of "the pluralist-elitist" literature (p. 13) but with the structures of the five nations that have made such literature irrelevant to their development. He does not believe, for example, that change in El Salvador has been sufficient to contribute "to the emergence or enforcement of a political culture of democracy" (p. 25). José Z. García's analysis leads to more sanguine conclusions. Taking sharp issue with the demonstration-election thesis, García believes the 1980s Salvadoran elections "had a profound influence in helping to alter some fundamentally undemocratic and unrepresentational Salvadoran realities" (p. 89). That argument might have been more plausible had the essay dealt more with the extent of U.S. financial and covert involvement in those elections and dealt also with the criticism that the methods of voting (allegedly in glass ballot boxes with the military observing) could have distorted the results.

Other essays are more critical. Robert H. Trudeau's superb analysis of the 1985 Guatemalan election concludes that the country cannot be considered democratic in the sense of the majority's being represented or of a lessening of authoritarian rule. It instead "institutionalized" authoritarian rule but ironically made it easier for U.S. politicians to send aid to the authoritarians (p. 118). Susanne Jonas rigorously works through a series of categories to demonstrate that Nicaragua is more democratic than Guatemala, although she adds that "Nicaraguan democracy cannot be discussed exclusively in terms of Western-style representative democracy" (p. 145). Mitchell A. Seligson and Miguel Gómez use Costa Rica for an instructive case study of the question whether a democracy, even one this deeply rooted, can survive the economic crises of the 1980s without paying an unacceptable price. Their important essay, read in the context of this book, raises the concern that perhaps so much blood and money have been spent in creating questionable and politically expedient democracies in the rest of Central America that the one true democracy, which should be a model for the rest of the region (and U.S. policy), has been fundamentally weakened.

WALTER LAFEBER

Cornell University

Germany's Past and Europe's Future: The Challenges of West German Foreign Policy. By Edwina S. Campbell. Washington: Pergamon-Brassey's, 1989. 237p. $33.00.

In the last paragraph of this book, Edwina Campbell cites West German president Richard von Weizsäcker's maxim that the German question remains open so long as the Brandenburg Gate is closed. Whether the obverse is true remains to be seen, but collapse of the Berlin Wall further reinforces the main point Campbell wishes to make: history has left West German policymakers convinced that borders that cease to be barriers lose much of their meaning.

Campbell argues that the Locarno pact of 1925, by which Weimar Germany pledged to respect the existing frontiers of Europe, has served as a model for the postwar federal republic's foreign policy. Yet since Locarno's spirit infused only a handful of statesmen, she contends, it could effect no genuine reconciliation; and Bonn's leaders thus adopted a "Locarno from below" approach to relations with France in the 1950s—diplomatic harmony reinforced by cross-border social and political contacts at all levels. This model proved so successful at overcoming the divisiveness of Western borders that it was applied to relations with the Soviet bloc (including East Germany) beginning in the 1970s. Herein lies the current appeal of *Mitteleuropa,* that much-discussed notion of an idyllic Central Europe—whatever its actual past or future configuration—in which frontiers have lost their power to divide.

Campbell argues that this approach to foreign policy has gained strength from the legacies of Auschwitz, Hiroshima, and Chernobyl: coming to terms with Nazi crimes, avoiding the risk of nuclear war, and dealing with technological interdependence has required a less state-centered approach to international affairs. Ironically, the one country that least shares this "new thinking" is the very neighbor with whom Bonn first sought reconciliation—France. In Campbell's view, the major task for Germany lies in winning its neighbor over to a concept of foreign relations predicated less upon the inherent antagonisms between states and aimed instead at creating a Europe-wide, multilateral Locarno.

Germany's Past and Europe's Future is thus not a comprehensive analysis of West German foreign policy to compare with Wolfram Hanrieder's *Germany, America, and Europe* (1989). It is rather a set of interrelated interpretive essays on the role history has played in the redefinition of Bonn's external relations. In two hundred pages, Campbell touches on and ties together almost every major issue within that broad theme. Accordingly, while there is considerable documentation, treatment of some questions is a bit light, and the amount of new research data is relatively limited except, to some extent, in the area of Franco-German reconciliation (where one might wonder if the role of such initiatives as the sister city agreements has not been somewhat overstated).

Nonetheless, the quality of the analysis and the writing make this an excellent guide to a complex matter, and events in late 1989 make the book's theme all the more timely. Campbell knows the topic from her experience as both practitioner and scholar, and her original insights into the subtle nuances of German diplomacy will be rewarding for generalists as well as specialists.

Moreover, while the author does not claim to have made any special contribution to international relations theory, on one level her book offers a convincing argument about the extent to which historical analogy governs the conduct of foreign policy. Auschwitz, Hiroshima, Locarno, Mitteleuropa, Chernobyl—in one way or another, all shape West German external relations in the way that Pearl Harbor or Vietnam still affect U.S. policy.

CLAY CLEMENS

College of William and Mary

The New Canadian Political Economy. Edited by Wallace Clement and Glen Williams. Kingston: Queen's-McGill University Press, 1989. 333p. $15.95 (Canadian) paper.

State Capitalism: Public Enterprise in Canada. By Jeanne Kirk Laux and Maureen Appel Molot. Ithaca, NY: Cornell University Press, 1988. 250p. $37.50 cloth, $11.95 paper.

Policy and Rationality: The Regulation of Canadian Trucking. By Harold Kaplan. Toronto: University of Toronto Press, 1989. 221p. $37.50 (Canadian).

In the 1970s and 1980s, Canadian social science began to diverge somewhat from the path of its U.S. counterpart. One point of departure was the growth of the new political economy school. Drawing on indigenous traditions of Canadian scholarship going back to the 1930s and 1940s—especially the economic history of Harold Innis—but infused with a strong Marxist tendency among younger scholars with a "left-nationalist" political bent, the new political economy has established a strong presence in sociology and political science (but not economics) departments.

In particular, the study of public policy in Canada is now fairly distinct from U.S. public policy approaches in the prominence given to theories and models derived from Marxist political economy. Even Canadian writing, which is liberal or pluralist in inspiration nevertheless tends to engage Marxist assumptions in debate; U.S. mainstream public policy study is, by contrast, almost wholly innocent of the concerns about class conflict and the relationship of sectors of the state to fractions of capital that animate Canadian debates.

The Clement and Williams collection, *The New Canadian Political Economy,* is an attempt to take stock of the state of the school at the end of the 1980s. The volume is above all a literature review and survey of some of the major points of debate within the new political economy tradition. Indeed, the relative vigour of the tradition is exemplified in the scope of these debates. In no sense can one say that the school has solidified into an orthodoxy or has grown sclerotic. Some prominent figures are represented in these pages, addressing some of the key questions that have arisen and reformulating, in some cases, their own positions. But, in addition, newer and younger voices bring newer themes ignored or downplayed in earlier years: Isabella Bakker on gender; Amy Bartholomew and Susan Boyd on law; Frances Abele and Daiva Stasiulis on native people and immigrants; Ted Magder on culture and communications. Any reader who wishes to acquire an introduction to, and overview of, the debates within the new political economy, as well as to gain some appreciation of the major contributions of the school, will find this volume helpful.

State Capitalism by Laux and Molot is a study of public enterprise in Canada and is also a case in point of the particular strengths of the political economy approach to public policy. Although the authors are by no means card-carrying members of the new "school," they are informed by its findings. They set out to explain a degree of state intervention in the market through direct ownership that is high relative to U.S. experience (although moderate in relation to many European countries). To do so they examine a series of case studies of federal and provincial state enterprises within a comparative framework stressing European experience.

They are particularly intrigued by the growth in recent years of a state presence in commercially competitive provision of goods and services and state investment directed by government development strategies, as opposed to the older practice of simply providing infrastructure and bailing out jobs through public ownership. They conclude that both liberal and Marxist interpretations are wanting and opt for an explanation that stresses the internationalization of capital as the key variable enforcing a change in direction on the part of Canadian (and European) governments. *State Capitalism* is an extremely well written and concisely argued treatise that will reward any readers interested in the future of public ownership. They have particularly illuminating observations to make on the paradoxes of public ownership, especially the inherent contradictions between market-led criteria of performance and the political criteria that have often spurred public enterprises in the first instance. They conclude that as governments have gone increasingly into business, they have increasingly been driven to become more "businesslike," that is, to behave in market-oriented ways.

This leads to a major weakness in the Laux and Molot argument, its failure to come adequately to terms with the privatization drive that has swept Britain and is now engulfing both federal and provincial public enterprises in Canada. Their book was published in 1988 before privatization crested. They are uneasily aware of the trend (which they see as related to fiscal constraints) but tend to downplay its extent and significance. In fact, a number of the enterprises they discuss have subsequently been sold. And the Canadian government has now signed a free trade agreement with the United States containing clauses specifically constraining Canadian governments, both federal and provincial, in the creation of new public enterprises. Perhaps the authors failed adequately to gauge the scope of ideology in the making of public policy.

Kaplan's *Policy and Rationality* shows that not all Canadian public policy study is inspired by political economy. In fact, Kaplan's study of trucking regulation does not even acknowledge the political economy approach. Instead, he sets out from a Parsonian/Weberian set of assumptions to challenge accepted notions of rationality in policymaking. His case study has not only led him to question the notion that participants in the process are engaged in a "seminarlike" problem-solving exercise, but also to question the public choice assumption that actors are capable of rationally calculating and carefully pursuing their individual self-interests. Instead, he proposes a set of interactive "games" that make up a specific social subsystem that is the policy process. This approach allows him to explain system malfunctions and pathologies arising out of the (nonrational) theatrical and ritualized behaviors of game playing.

Kaplan argues with wit, verve, and some force. Although the case study is Canadian, the theory is addressed to the mainstream of U.S. public policy studies. He points to another, neglected, layer of analysis. As such, his book demands close attention. Without gainsaying Kaplan's contribution, one wonders whether his argument would not have been strengthened by a recognition of the underlying economic structural constraints on the field in which the policy games are played out. Here, the Canadian political economy school might have been of some assistance.

REG WHITAKER

York University

Portugal: O sistema politico e constitucional, 1974–1987. Edited by Mario Baptista Coelho. Lisbon: University of Lisbon, 1989. 1044p.

For scholars interested in transitions from authoritarian rule, especially in Southern Europe, this is a significant publication for several reasons. First, it constitutes the most comprehensive discussion of Portuguese democracy in print. Second, it opens up new and important insights into the protracted transition that occurred in Portugal. Such vistas can be especially instructive for those who have their doubts about the prospects for democracy in societies with a legacy of authoritarian rule, politicized military and security establishments, and a history of restricted political participation. Third, it signals the emergence of a distinct community of Portuguese scholars concerned with the analysis of their own polity in their own terms. In bringing together those who have contributed the most to the development of the social sciences in Portugal over the last two decades, this compendium captures the transactions between state and society in Portugal in such a way as to supersede what others from abroad have written about their politics and the transformation that society has undergone since 1974.

Whereas the prior literature deals with segments of Portuguese experience since the early 1970s—the Armed Forces movement, political parties, elections, mass movements, various economic and political transformations—this volume brings together what has been by definition a disparate set of experiences. Its very organization establishes the kind of overview of Portugal's new democracy that has been lacking until now. Beginning with a discussion of the regime and the transition that has taken place, it surveys the parties and the multiplicity of views on politics that have emerged. Especially notable in the latter section are Jose Pacheco Pereira's insightful analysis of the Portuguese Communist party, Antonio Reis's survey of the evolution experienced by the Socialist party, and Pedro Santana Lopes's discussion of the Social Democratic party's dependency on charismatic leadership. What follows immediately thereafter is a set of articles on elections and the party system. Warranting special attention here are the discussions provided by Mario Bacalhau and by Isabel Andre and Jorge Gaspar.

This book brings in fresh, original perspectives on the Portuguese political system in its discussion of Portuguese governance (especially the chapters by Francisco Lucas Pires and by Manuel Braga da Cruz and Miguel Lobo Antunes) and the intermediate groups and institutions that have structured the transaction between state and society. Antonio de Sousa Franco's essay on the church and its influence on power relationships, Jose Medeiros Ferreira's analysis of the military corporation and the primacy of civil-military relations in determining political outcomes until 1982, and Antonio Barreto's emphasis on the role played by the state and the various groups vying for control of the state apparatus in determining the outcomes of agrarian reform all constitute important revisions of conclusions hitherto reached about democratic rule in that country. Equally as significant as these three chapters in revising our understanding of democratic rule in Portugal is Diogo Freitas do Amaral's discussion of constitutional revision and the settlement reached between military and civilian authorities in 1982 over military disengagement from politics and the acceptance of civilian primacy.

Of less interest to the political scientist is the extended attention given to the constitutional system and the analysis of the new regime's basic laws, its rights and guarantees, legal constraints on the economy and society, and constitutional revision through judicial interpretation and legislative revision. This is especially the case given the long delay between completion of these articles in 1987 and their publication in 1989 at the very time a second set of major constitutional revisions were underway. While there is ample discussion in this volume of the constitutional revisions adjusting civil-military relations in 1982, the reader will find nothing of the equally important adjustments in the relations between the state and the economy in 1989 through the last round of constitutional revisions in May and June. Nevertheless, when this aspect of the volume is compared with the overall contribution represented by the 49 participants in this project, this is a minor point. For what is most significant about this particular book is the representation and visibility it gives to Portuguese scholars—many of whom have pursued simultaneously active political roles in building Portugal's new democracy—as the primary commentators on their country's politics and government.

Lawrence S. Graham

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Beneath the Miracle: Labor Subordination in the New Asian Industrialism. By Frederic C. Deyo. Berkeley: University of California Press, 1989. 273p. $30.00.

What has made the economic "miracle" possible in Asia's newly industrializing countries (NICs)? Most scholars have attributed the remarkable development of Taiwan, Hong Kong, Singapore, and South Korea to a combination of economic, political, and cultural factors.

Economically, the impressive growth has been achieved as a result of rapid expansion in manufactured exports. With the exception of Hong Kong, the three other Asian NICs went through a stage of import substitution industrialization before they entered the stage of export-oriented industrialization with high growth rates. On the basis of these experiences, economists conclude that economic promotion is a better policy for economic development than import substitution.

In addition to the choice of "right" policy and the effective implementation of such policy, noneconomic factors such as Confucian ideology and the nature of authoritarian political systems have contributed much to the success of the Asian NICs. Social scientists believe that these factors are highly conducive to economic growth and facilitated the implementation of export promotion policies.

The present book presents a new explanation. As suggested by the title of the book, Deyo sees the economic miracle created by the four Asian NICs as based upon a relatively weak Labor movement. Unlike labor unions in Western capitalist countries and even in the South Americas, labor unions in these Asian NICs tend to be unmilitant, giving the management more room in which to operate their enterprise and lowering cost for labor. The impotence of labor unions in these Asian NICs can be proved not only by a smaller number of labor disputes but also by the nature of these disputes. Deyo asserts that when a conflict between the employees and the employers occurs, often it is not because the employees demand an increase in their salary or benefits but because the employers cut their salary or benefits.

Furthermore, labor unions in East Asia play an unimportant role in politics. Often, coopted or repressed by the government, leaders of labor unions are totally subordinated to the will of the elites. Labor unions in these NICs, especially in Taiwan and Singapore, served more or less as instruments of their government for controlling workers, thus losing the true meaning of labor unions.

Research on Labor movements in Asia has been a weak link; thus, Deyo's probing study is highly welcome. He analyzes the evolution of Asian Labor movements in historical, cultural, political, and economic contexts and uses Confucianist culture, economic developments, and political systems of these Asian NICs to explain their weak Labor movements.

It is undeniable that labor subordination has contributed to rapid economic development of the Asian NICs (and, for that matter, to the industrialization of England and the U.S., historically). However much the author may highlight labor subordination in the history of Asian economic development, he should not exaggerate its role; for the weakness of the Labor movement *alone* does not result in the high economic growth of these Asian states.

We should also point out that the Labor movement is rising and that the workers are playing increasingly active roles in politics in both Taiwan and South Korea in recent years. An interesting question is whether these states can continue their rapid economic growth in the face of expanding organized labor activities in the years to come.

Parris H. Chang

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The Myth of the Welfare State. By Jack D. Douglas. New Brunswick, NJ: Transaction Books, 1989. 505p. $34.95.

In Care of the State: Health Care, Education, and Welfare in Europe and the USA in the Modern Era. By Abram de Swaan. New York: Oxford University Press, 1988. 352p. $39.95.

In the wake of myriad failures to achieve promised ends, seemingly endless budgetary increases, more than occasional worsening of problems by state intervention, and impingement on individual freedoms, citizens, government officials, and social scientists alike have begun to question seriously the efficacy of bureaucratic solutions to social and economic problems. Remedies proposed range from imposing tighter external controls over bureaucratic behavior and forcefully improving efficiency to reverting to private market mechanisms for addressing many problems now handled in the public domain by the state.

No appropriate solution may be found, however, unless we ask and provide a minimally plausible answer to the question, How did we get here from there? Welcome, then, are books such as these by Douglas and de Swaan that seek to understand the developmental sequence that has wrought the modern welfare state. Both have great scope, proceed from a macro perspective, and address the issue in various cultural contexts and over a lengthy historical period, enabling them to rise above studies limited by the idiosyncracies of specific places and times.

Proceeding from the belief that most "work of the social sciences [is] built on misinformation and disinformation, including the carefully constructed theodicies and propaganda of some statist bureaucratic information" (p. vii) and "concerned with the big picture" (p. 13), *The Myth of the Welfare State* attempts from an " 'everyday life' perspective" (p. viii) to explain the "great tidal drift into megastatism" (p. 19).

Relying upon analysis of existing historical studies and arguments by economists and political theorists, Douglas contends that the relentless movement toward "megastatism" may properly be explained by four core factors: the ancient myths of statism, especially that of the "common welfare" (p. 179); an ignition factor, the "myth of secular millennialism" (p. 179); the tremendous growth of "rationalism, technicism, scientism, expertism" (p. 183); and "autocatalysis or positive feedback" (p. 184).

In good measure, Douglas concludes, the welfare state has developed and been sustained by piecemeal decisions made under the influence of myths that have served to alter the public's conception of the costs and benefits of such an approach to problems. The cumulative effects of what may have been individually beneficial incremental decisions have been the grave dangers of government power and the immorality of the infringements on liberty they entail. The only way to reduce these costs is to cut through the mythology and reduce our dependence on government programs voluntarily. Implicit in Douglas's argument is the suggestion that when the choice comes down to living with problems unresolved by the market or seeking some form of government intervention, we ought usually to opt for the former, as these costs will be more bearable than the costs of more government.

Ultimately, however, *The Myth of the Welfare State* does not satisfy. Despite basic sympathy with the book's premise that the expansion of the state has brought us far fewer benefits than expected and far heavier costs than ever anticipated, I find this work problematic. It reads more like a disjointed, angry polemic than a carefully reasoned and documented effort to come to grips with a crucial issue and offer some pragmatic recommendations for its resolution. Douglas is unabashedly antistate, antibureaucracy, and promarket. Certainly, polemics have proven useful in the social sciences: they stimulate debate, causing us to think about problems we had not previously considered or to think about old problems in new ways. However, *The Myth of the Welfare State* only partially achieves these ends, treading paths earlier (and perhaps better) explored by others, such as Hayek and Popper on the one hand and Merton, Simon, and Lindblom on the other. It thus increases our corporate wisdom only marginally and takes a great many pages to do so. Its language, at times overwrought, intrudes: simplicity would enhance the forcefulness of the book's argument. Use of evidence is inconsistent. For example, on one page Douglas condemns as fraudulent the findings of anthropology but on another page employs the same to support a point. And although theoretical parsimony is an important aim in any study, this book oversimplifies what is surely an uncomfortably complex reality. In so doing it misses the wide variety of causes contributing at different times, in different combinations, to the rise of the welfare state.

Douglas decries the welfare state without offering an explanatory theory that is sufficiently rich and at the same time so qualified as to permit us to select from among alternate future courses of action. A pell-mell flight from statism to the market will no more guarantee solutions to complex social problems than the obverse. As Herbert Simon has observed, some of our problems may simply not be amenable to solutions of any kind.

Although concerned also with the increasing control of the central state and its bureaucratic apparatus over more and more intimate aspects of life, working from a macro perspective, and relying on secondary analysis, de Swaan has produced a very different book from Douglas's. With collective action and comparative historical sociology providing its theoretical foundations, *In Care of the State* seeks to answer the question, "How and why did people come to develop collective, nationwide and compulsory arrangements to cope with deficiencies and adversities that appeared to affect them separately and to call for individual remedies?" (p. 2).

Throughout, theory is carefully grounded in the history of the societies studied (Britain, France, Germany, the Netherlands, and the United States), with state formation, the rise of capitalism, and urbanization and secularization forming the historical background. Specifically analyzed are the etiologies of charities and national assistance, elementary school curricula, public health policy, worker's mutualism, and social security. The latter part of the book also considers the effects of bureaucratization and professionalization on cognitive orientation, affect management, and the emergence of social consciousness. In the broadness of its sweep and reach, along with its historical method, *In Care of the State* may be compared with the work of Max Weber and Reinhard Bendix. Further, de Swaan endeavors to synthesize the two theoretical perspectives, in so doing lending to formal theory a capacity for dealing with process.

De Swaan argues that collectivization of care arrangements has proceeded along three dimensions: scale increases, arrangements become more collective, and are more and more carried by the state. Such large-scale, collective, and compulsory arrangements developed more readily when certain conditions applied: uncertainty as to the moment and magnitude of adversity, uncertainty as to the efficacy of remedies against adversity, and the extent and reach of the external effects of adversity and deficiency on others not directly afflicted themselves. It is de Swaan's thesis that while these conditions may be more effectively managed in large-scale, collective entities, at the same time the dilemmas of collective action are sharpened because they diminish mutual control and reduce common solidarity. This means that the intensity and scope of external effects depend largely on the density and extension of the social network; that is, the degree of social interdependence determines "the consequences of adversity and deficiency of some of them upon others, not directly afflicted themselves" (p. 8).

Throughout, de Swaan writes clearly, persuasively, and to the point. The argument is presented in a novel fashion (especially his discussion of vagrancy, property, and charity); and though the depth and quality of analysis varies by substantive focus, on the whole it captures the subtleties and nuances of state formation and collectivization in an elegant and useful manner. By shaping an explanatory theory that penetrates to the commonalities underlying these various domains across both time and culture, de Swaan achieves generalizability without losing sight of the complexity of the problem at hand. Adherents of public choice theory would do well to mark this book's careful attendance to empirical evidence. Less immediately focused upon a reform agenda than Douglas (as the author himself notes, his book "hardly contains polemics" [p. 8]), de Swaan has, by the strength of the relationship between his theory and his data, contributed more substantially to our causal knowledge and thereby to our ability to choose from among alternative future courses and to estimate the probabilities of success of such choices. *In Care of the State* is an important book that has much to say to economists, political scientists, and policymakers concerned with practical reforms of prevailing approaches to difficult social problems.

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Nicolae Ceauşescu: A Study in Political Leadership. By Mary Ellen Fischer. Boulder: Lynne Rienner, 1989. 325p. $42.50.

This book is the fruit of many years of research, some spent in Romania. The author makes extensive use of Romanian sources (mainly newspapers) and of all the pertinent English language literature. The French—and especially the very extensive German—literature on the subject would have been helpful. The focus of the study is fairly narrowly on Ceauşescu in the context of the party. Because Ceauşescu was a relatively small fry in the earlier period, the first few chapters provide a broad picture of the entire party leadership and the struggles within it. These struggles were easier to reconstruct because the party had not yet clammed up, as it was later to do; and more of its inner affairs percolated out in official and nonofficial ways. In the 1970s and 1980s the party and Ceauşescu (plus his wife and immediate family) became practically identical; thus, Fischer's focus narrows, and she devotes considerable discussion to the Ceauşescu leadership and cult of personality. Still, even in the final chapters, she offers some supplementary perspectives—for instance, in a discussion of the Grand National Assembly and the experimentation with more than one candidate at election time in the 1970s. The conclusions are off by a wide mark, given what happened in December 1989. But did anybody guess better?

By and large one can say that Ceauşescu fares well in this book in the sense that he is taken seriously. His policies—especially the results of the policies—are not in any way defended but are exposed for what they are. As a person and political leader, however, he is portrayed not as a dictator interested primarily in his own well-being but as a man pursuing specific goals for his nation. He is depicted as guided by fairly clear visions of the future within the framework of a worldview, however ill-conceived and abhorrent to the Romanians they may have been. At one point (p. 32) the author even draws a close parallel between Ceauşescu and Walter Ulbricht. Both of them, she claims, had their thinking molded by a kind of "narrow-gauge Marxism" even if they may have lacked "the basic tools for thinking."

The comparison seems far-fetched; for whatever thinking problems Ulbricht may have had, he certainly had read his Marx, Lenin, and Stalin. Every German Marxist, especially one of the generation to which Ulbricht belonged, was extensively grounded in Marxist literature, no matter at what level he was able to absorb it; and a German communist who spent the 1930s and early 1940s in the Soviet Union also read a lot of Lenin and Stalin. Nothing of a comparable theoretical or ideological baggage encumbered Ceauşescu, who as a young, barely literate peasant learned his Marxism (or only Leninism and Stalinism) in a few snippets passed on to him by his jailmate and teacher, Gheorghiu-Dej, himself hardly more educated. As a matter of fact, more extensive Marxist literature in the Romanian language, the only one that Ceauşescu or Gheorghiu-Dej could read, did not become available until the communists were entrenched in power and Ceauşescu was already fairly high on the party ladder.

The roots of Ceauşescu's worldview (if his disparate ideas deserve such a designation) is more likely a direct offspring of peasant awe for machinery and all it can do that village tools can never match. In other words, for Ceauşescu the importance of industrialization lay mainly in what it could directly produce, not in what Marx claimed are the long-range historical transformations it causes in man, society, and the man-nature relationship.

As a reference for the study of Romanian party politics, this book is a gem, but it does not add much to a deeper understanding of Romania and the Romanians. I, for one, did not find in it any explanation—or even attempted explanation—of why communism in Romania assumed the forms it did, so different from those in neighboring countries. Occasionally it seemed that the author wanted to broach this issue—for instance in her exploration of the character of Ceauşescu's leadership—but the attempt fizzled out in a good theoretical discussion of charisma, without concrete and credible conclusions as to cause and effect in the particular case in hand. Most distressing is the absence, in the book, of the Romanian people, the Romanian nation with the whole weight of its history, culture, and traditions. The party seems to swim above; but one is not sure in what it swims, what makes it swim, or what may cause it to go under. The rather long discussion about charismatic leadership would have been more useful had there been an attempt to weigh whether Romanian culture is at all compatible with such leadership. It seems, in fact, that it is not. The only man with genuine charisma in the recent Romanian past was a university professor, Nae Ionescu; but the range of his charisma was limited to a few hundred, or at maximum one or two thousand individuals. The Iron Guardists might want to make a claim for their leader, Corneliu Codreanu, but it is questionable to what extent his charisma was genuine and to what extent manufactured. The simple fact is that Romanians, by nature or through experience, tend to be impervious to ideologies or anything that is not down to earth, in the realm of common sense. Anything high flown, too high for the common man to see and touch, must be taken with a grain of salt. That includes religious beliefs. Charisma just bounces off the average Romanian like water off a duck's back.

LADIS K. D. KRISTOF

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Northern Ireland: The International Perspective. By Adrian Guelke. New York: St. Martin's, 1989. 236p. $39.95.

Northern Ireland: The Political Economy of Conflict. By Bob Rowthorn and Naomi Wayne. Boulder, CO: Westview, 1988. 230p. $45.00.

Consensus in Ireland. Edited by Charles Townshend. Oxford: Clarendon, 1988. 214p. $45.00.

The conflict in the United Kingdom's Irish periphery receives broad analytic attention in these three books. They may serve to counter the attitude prevalent in U.S. academia that the instability is atavistic or inscrutable, or both.

Innovatively examining the conflict's international dimensions, Guelke offers a painstaking chapter on pro- and anti-Irish Republican Army (IRA) Irish ethnic groups and British and Irish regime lobbying in the United States and U.S. governmental and political party reactions in the 1970s and 1980s. Guelke notes circumstantial evidence of the Reagan administration's aid to the Friends of Ireland–Dublin camp with respect to encouraging British initiatives, notably the move towards the Anglo-Irish (Hillsborough) Agreement of 1985.

Guelke's reference to neo-Fenians as "the Irish-American lobby" obscures the role of the Dublin-aligned Friends, and their middle-class allies. This network has been the dominant force; its silence allowed the Senate to endorse antiasylum changes in the Anglo-American extradition treaty, which the greener lobby bitterly opposed. Guelke's claim that there is a general U.S. disposition favoring a united Ireland is not supported by any major evidence. Clearly, foreign policy elites have rarely expressed such an opinion. Anglophilia and Hibernophobia have had something to do with the condition, as have undifferentiated antiterrorism with Israeli roots and Irish nationalism's long-term slide everywhere.

The Guelke book assumes, rather than demonstrates, that the conflict has been internationalized in some broad sense. There has been sporadic Soviet and other European nettling of London; Dublin has brought cases into the European human rights network; the European Parliament has ventilated and studied the conflict. These interventions have been very limited. Only Libya and liberation groups have endorsed the IRA, which is becoming increasingly isolated. Dublin has never taken the partition issue to the United Nations or the International Court of Justice or called for a multilateral conference on Irish issues.

The Irish Republic has sought to protect the interests of the northern minority. Angering the Protestant unionists, the Irish Republic has achieved consultation-plus access to British policy toward Northern Ireland through the Hillsborough treaty. Although recommended by Martin Dent in Townshend's edited volume, joint sovereignty has not been established. London retains full authority.

Importantly, the 1985 accord strengthened the built-in unionist majority's veto over any serious move toward unification. Part of a trade-off, the enhancement derived from the FitzGerald government's treaty acceptance of the principle that there was to be no change in Northern Ireland's status without the consent of a majority. Dublin's acceptance enables London to invoke "democratic self-determination" to justify partition. The effort was undermined by the 1920 gerrymandered creation of Northern Ireland without the consent of the island's people and against insular majority electoral wishes registered in the 1918 general election.

Examined in Brian Farrell's foreword and Bernard Crick's and David Trimble's chapters in the Townshend book, the "consent doctrine" stems chiefly from (1) the 1949 British guarantee of Northern Ireland's place in the United Kingdom and (2) Irish revisionism's denial of Anglophobic republicanism embedded in the deValeran Irish Constitution's "irredentist" articles in favor of an educational strategy toward the unionists. Vital for Hillsborough's inclusion of the majority-consent doctrine was the 1984 shift of John Hume, leader of the North's mainly Catholic Social Democratic and Labor party, away from earlier demands that London renounce the guarantee. Withstanding British, unionist, and Irish revisionist calls for their deletion or blunting, the offending articles have now been overshadowed by the Hillsborough's incorporation of the consent principle, a step found legal in Irish courts.

Neither Farrell nor Crick find Hillsborough's encasement of the majority-consent doctrine remarkable, which it is, in the perspective of traditional Irish nationalism. Axiomatically, the incorporation is contrary to the militant republicanism of the IRA-supporting Sinn Fein, with only 5% insular electoral support. Not only Sinn Feiners or their associates (as Crick says) have taken the view that unity is not negotiable while the means to reach it are open to bargaining. That was the posture of Charles Haughey, leader of the island's largest party, Fianna Fail, during Dublin's debate of the treaty. Finding the treaty widely popular, Haughey submerged his republican objections to the consent formula in the pact and worked the new Anglo-Irish institutions after he returned to power in 1987.

The treaty has continued to be popular in the Republic, which has a low-intensity but majority support for unification. The South has legal bans on abortion and divorce, thereby deepening partition and dismaying pluralists, among whom the Catholic Church is not yet to be counted. These books contain scattered references to church-state issues.

Essentially satisfied with the 1985 agreement, Crick views it as a move not towards unity but "towards a plateau of wider acceptance of institutions of government" (p. 127). This may be understood as an argument for devolved power sharing; it was tried, but a loyalist strike destroyed it in 1974. John Darby's chapter in the Townshend book argues for this kind of bridge building.

There is majority support within each of the Northern communities for power sharing to replace London's direct rule; but ultraloyalists have kept the two unionist parties from moving in that direction unless they can gain Hillsborough's suspension or radical alteration. Thatcher policy will not permit such a development, thus dimming the prospects for power sharing.

Unlike the other two volumes, the Rowthorn and Wayne study projects orthodox republicanism. Supplying descriptive chapters on the conflict's background, the book also supplies socioeconomic data showing disparities between Catholics and Protestants in the North, and between North and South. Unlike many British and Irish laborites, the authors do not view socialist reform of Northern Ireland as the answer to the conflict. Holding classic nationalist assumptions—expressed by Haughey out of power—Northern Ireland is held to be unreformable. No nationalist, Townshend himself speaks of Northern Ireland as a polity created in a terminal state. According to Rowthorn and Wayne, the solution lies in Britain's sovereign withdrawal.

Endorsed by Sinn Fein and a left-Irish faction of the British Labor party, this disengagement thesis refuses to accept a widely held doomsday scenario, that intercommunal violence would soar if Britain withdraws. The British "Time To Go" movement faces the anti-disengagement stances of all British and Irish political parties, save Sinn Fein, as well as the Dublin and London regimes. Consequently, the orthodox republican prescription faces enormous obstacles.

Guelke compares Northern Ireland's conflict and other regional instabilities, as does James O'Connell in the Townshend book. But the earnest efforts fail to overcome the usual comparative problems of divergencies of culture, ideology, place, resources, and time. John Whyte analyzes the body of writings on the conflict; but his method of counting modes of basic assumptions about the primary source of conflict (e.g., Catholic versus Protestant, capitalist versus workers, and Britain versus Ireland) is an inadequate substitute for judging which interpretations have persuasive explanatory power and which do not.

A comprehensive analysis of the literature dealing with the conflict, now over two decades old, has yet to appear. The same may be said about a master account of the overall contestation perpetuated by complex stalemates.

PAUL F. POWER

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Rebellion and Repression in the Philippines. By Richard J. Kessler. New Haven: Yale University Press, 1989. 227p. $25.00.

This would be an important book even if its author were not "a professional staff member responsible for Asian issues" of the Senate Foreign Relations Committee. The importance of *Rebellion and Repression in the Philippines* far outweighs the strategic location of its author, however. Kessler does us an important service in clearing away the romantic illusions that may have overtaken us during the brief euphoria of the "People Power" revolution of February 1986. His basic message is that the Aquino administration has reestablished traditional personalistic politics to the Philippines and presided over the loss of initiative for reform. Aquino stood by as the reconstituted premartial law system fell once again into the historic Philippine pattern of relying on repression rather than adaptive structural change when faced by a rebellion.

The book, furthermore, should be read for Kessler's interpretation of Philippine history, namely, that it has a historical culture that generates rebellions, to which elites respond by employing state (military) repression. He builds this model by extracting themes from an extensive literature on specific peasant, religious, and nationalist revolutions in the past, combining them (as if laid down in sedimentary layers) with a view of traditional cultural values that has long dominated mainstream analysis of Philippine society. The model is heavily freighted with cultural variables and with a near-total focus on internal forces at the expense of an analysis of any role that the United States, for example, might have played in creating an elite of the type he describes or a military that finds its primary role in supplying internal repression. His carefully crafted substantive chapters on the current cycle of rebellion and repression are built on this model.

Those chapters deserve careful reading. The ones on the Communist Party Philippines–National Democratic front–New Peoples Army (CPP-NDF-NPA) and on the Armed Forces of the Philippines provide excellent insights that are often stunning in their challenge to conventional readings of the contemporary Philippine struggle. Most importantly, he sees the CPP-NDF-NPA as successfully achieving what no other rebellion (or political party) historically has, namely, creating a national, disciplined force that has largely overcome personality politics and has a demonstrated track record and that continues to appeal to large numbers of peasants despite its blunders. His telling conclusion (after noting that the AFP–Aquino government has achieved none of these) is that "the party had evolved into a political organization unlike any previously known in Philippine history" (p. 104). Elsewhere ("A New Philippine Political System," in *Rebuilding a Nation*, ed. Carl H. Landé (1987), he had made it clear that he believed that the future of democracy in the Philippines depended on whether Aquino would be able to build a "new party along modern lines" that would be able, "in effect . . . to disenfranchise these [traditional] elites unless they developed their own modern party to compete" (p. 377). This has not happened.

His second set of contributions relates to his relentless examination of the myths about the AFP. He does not consider it the detached, "professional" force many have made it out to be but rather as an organization perennially ordered along personality chains and deeply involved in politics. He finds that all reform moves have failed and that it historically has functioned "as the primary tool to protect elite interests . . . [and] to frustrate social reform" (p. 105). While it might be able to overcome the current rebellion (though Kessler seems doubtful) through repeating the cycle of repression, a new attack on the system would certainly occur, rather sooner than later.

At this point Kessler proposes that the only way the CPP can be defeated would be for the United States to make a greater effort in helping to promote institutional change and to push the elite "to sacrifice its welfare for the sake of the nation's" (p. 153). He is, of course, cautious about the prospects for success in trying to use U.S. aid "to effect structural change" in the Philippines, as such attempts have "failed in the past" (p. 157). His proposals largely flow out of the congressional scheme for a mini-Marshall Plan (then called a Multilateral Assistance Program, and, when agreed to in Tokyo in July 1989, the Philippine Assistance Program) for the Philippines (p. 159). Kessler's absolute resistance to recognizing a U.S. role in the creation and reproduction of the very elites and AFP that he so severely and correctly castigates makes him blind to two intractable problems. First, if the United States has been and remains a powerful force in producing the structures that are flawed, how—short of military occupation—can it induce the structural changes and the creation of new institutions he sees as so urgently needed? Secondly, if U.S. security and economic interests are as great as he suggests (on which I concur), what leads to his belief that the United States would ever sacrifice them to the needs of "structural reform"? Unfortunately, he cannot answer these questions. Asking them in the first place would have, of course, made for a quite different set of conclusions.

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The Logics of Party Formation: Ecological Politics in Belgium and West Germany. By Herbert Kitschelt. *Ithaca: Cornell University Press,* 1989. 343p. $44.95.

The literature on European environmental politics, particularly the German Green party, has grown considerably over the last few years. Not surprisingly, most of the research has focused on the Green parties as representatives of the new social movements. As unconventional challengers of the establishment mass parties who nevertheless had decided to play the party game, the Green parties were expected by most either increasingly to resemble conventional parties or else break apart under the burden of accomplishing the impossible.

Kitschelt, drawing on his own original research as well as existing literature, has succeeded in elevating the discussion of the German Green party especially to a new level of theoretical sophistication and comprehensiveness. His thesis is that a proper understanding of the Greens challenges the "conventional functionalist' view of party organization" (p. 4), with its theory of systemic imperatives. Thus, the intent of his study of Green parties is to challenge "structural" approaches to party organization and rational actor-oriented studies of parties. Putting the left-libertarian Green parties in the context of a European political transformation of electoral realignment and organizational dealignment between citizens and parties, Kitschelt makes his case by distinguishing between two logics of party formation. In contrast to the conventional "logic of party competition" pursued by conventional mass parties, the Greens are innovatively using the "crisis of participation in many modern democracies" (p. 5) for a new "logic of constituency representation."

Under this rubric Kitschelt provides a theoretical analysis of all the characteristics of the Green party associated with its attempt to be a "nonparty party." The most interesting aspects of his analysis emerge from the failure of the Greens' "new vision of interest intermediation between citizens and state" easily to translate into a new party organization and strategy. Indeed, certain unforeseen "perverse effects" result that disable the Greens in the pursuit of either of the two logics of party formation. One major "perverse effect" merits emphasis, since it goes to the very heart of most Green parties' commitment to participatory democracy. Kitschelt argues that false assumptions about the attractiveness of political participation, as well as the role of resource scarcity in participation in general, leads to the rise of competing cliques of political entrepreneurs dominating the distribution of power in ecology parties. Violating the precepts of participatory democracy and egalitarianism valued by Green activists, this development, while preventing the centralization of power, leads to widespread dissatisfaction among the rank and file.

In a series of chapters comparing the experiences of the Belgian and German Green parties, Kitschelt provides ample evidence for his final conclusion, namely, that "left-libertarian parties develop loosely coupled, fluid organizations that lack cohesiveness, involve only a small number of activists, establish tenuous, intermittent linkages to social movements and interest groups, and build party leadership on postmaterialist political entrepreneurs" (p. 277). There is no space here to do justice to the richness of his evidence and the logical consistency of his argument. In my judgment, his analysis of party activism is exemplary; his treatment of the German Greens' party conventions and the "irrelevance" of party organizational structure is particularly useful. He is right to emphasize the problem of unaccountability, weak rationalization, and legitimacy of collective choices in the fluid organizational structure of the Greens. But I could just as easily point to his tight argument concerning "the politics of alliance formation" as meriting special attention.

Interestingly, Kitschelt argues, in a sense, against his own thesis that in the final analysis the Greens are caught in the "dilemma" that "lack of strong party organization hastens the adoption of a logic of party competition" (p. 274). Given external systemic constrictions and conditions, in other words, the Greens' task of new consistency representation faces enormous obstacles. The temptation is great for them to escape their difficulties by joining the conventional game of electoral politics through skilled alliance politics. Despite the impossibility of predicting the future development of left-libertarian parties, Kitschelt ends by venturing some thoughts on their general importance for contemporary theories of democracy. For the moment it appears that mass parties have exhausted their abilities to integrate new political demands into existing party organizations and mediation patterns. Here lies a continuing role for Green parties. Perhaps one serious lack in this study is its general failure to examine environmental politics in contemporary Western societies and to what degree its adoption by mainstream parties might undermine the future role of the Greens. Regardless, Kitschelt's superb study is essential for an understanding of the present dilemmas of German and European Green parties and their place in modern party systems.

HORST MEWES

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The State and Poverty in India: The Politics of Reform. By Atul Kohli. New York: Cambridge University Press, 1987. 260p. $42.50.

This book is concerned with a major moral question—why does rural India remain impoverished after four decades of developmental effort? The analysis is pursued in terms of the impact of regime variation on distributional outcomes in India; the result is consequential.

The main burden of the study is a comparison of regime performance under three different party governments in three major Indian states: the Communist Party of India—Marxist (CPM) in West Bengal (1977–84), the Congress government under Devraj Urs in Karnataka (1974–80), and the Janata government in Uttar Pradesh (1977–80). These units are enormous and varied and suggest the magnitude of comparative study within India, in that if any one of these states were sovereign it would rank among the most populous nations of the world. The cases are nested among comparative analogues; while relevant to developmental studies generally, the power of the book rests primarily in its elucidation of the complex Indian situation.

Kohli elaborates the difference between the party regimes in terms of their social base, ideology, and organization. The CPM is socially and institutionally autonomous, ideologically firm in its commitment to agrarian reform, and has developed strong grassroots organizational and institutional ties that have been employed to mobilize and channel the energies of the socially dispossessed for common betterment. The Congress party in Karnataka during the period under examination was the creature of a consummate, populist leader who endeavored to fashion an electoral party with a new base of social support (a coalition of not-traditionally-dominant propertied groups together with "backward castes") and professed an egalitarian ideology, but whose party lacked organizational scaffolding. The Janata, on the other hand, was a five-party coalition formed to contest the 1977 elections that had brought to an end Indira Gandhi's so-called "national emergency." It ostensibly represented the interests of the middle and upper peasantry, but had no organization outside the informal structures of support enjoyed by each partner of the coalition separately. Policy effort is explored in terms of land reform (principally tenancy rights), assistance to small farmers, and enhanced opportunity to landless laborers.

The CPM is found to have been most successful in instituting and implementing agrarian reform. While deliberately avoiding a policy devoted to redistribution of land above the state's specified ceiling, the CMP instituted a program of rights and protection for sharecroppers (*bargardars)* that placed the burden of proof on landlords to establish that claimants did not have occupancy rights rather than on the tenant (the ineffective procedure of the past). The CPM was able to employ a coalition of party workers, peasant associations, and local government institutions (*panchayats)* to investigate, help authenticate, and monitor the implementation of rightful claims and was instrumental in expanding access to credit for small farmers and to additional employment for landless laborers. In Karnataka agrarian reform was more muted, not so much from a lack of commitment on the part of the chief minister as from propertied groups' being able to influence the implementation of tenancy rights through their participation in local tribunals created and empowered to hear and judge evidence on tenants' claims and issue binding decisions. Reform there was, however, though Kohli found it differentially distributed, in that it was most effectively implemented on lands owned by groups outside the support base of the governing party. In Uttar Pradish, on the other hand, the author found no progress whatsoever in agrarian reform nor evidence of any commitment to it. This he sees as a conseqence of the a middle-upper peasant caste coalition that constituted the factional core of the Janata party, whose interests were to deflect reform rather than to promote it.

While containing a number of interesting observations and propositions, the principal conclusion of the study is that under circumstances such as prevail in India ("electoral-constitutional rule and a private-enterprise economy") a successful reformist regime will be characterized by "a coherent leadership; an ideological and organizational commitment to exclude the propertied interests from direct participation in the process of governance; a pragmatic attitude toward facilitating a non-threatening as well as a predictable political atmosphere for the propertied, entrepreneurial classes; and an organizational arrangement that is simultaneously centralized and decentralized" (p. 10).

This study has many strengths. It addresses one of the most fundamental issues of social and economic policy in contemporary India. It is one of only a handful of studies that attempts systematic comparison of action and outcomes across important political units and the isolation of those conditions conducive to the development of state autonomy. The design and effort are commendable, the findings provocative and useful.

While theoretical in format, the study is equally driven by Kohli's being profoundly disturbed by India's persistent poverty and his consequent concern for determining "who is to blame" (p. 6) for this state of affairs. In his judgment, the failure of land reforms in India "resulted from a failure to institutionalize elements of lower-class interests within the state . . . primarily a failure of the . . . Congress regime under Nehru" (p. 69). Resistance to reform, he suggests, is reflected in the failure to reform a colonial bureaucracy, the commitment to constitutional protection of the right to private property, and the dependence of Congress on local notables as a consequence of electoral necessity. A strong nationalist leadership, a tightly organized party, and preparedness to use a measure of compulsion in a situation of weak commercial groups and retreating landed elites at the time of independence could have resulted in a more egalitarian India today. Kohli would have willed things differently.

Some readers will ask for more systematically comparable data, and will question some of the inferences extrapolated from the data provided. For example, the Congress-Urs regime in Karnataka is submitted to harsh judgment for not pursuing a land redistribution policy, while the same decision in West Bengal is understood more benignly. Indeed, data provided (pp. 121, 165) would suggest that Karnataka has had a better record of land redistribution than West Bengal. It is difficult to determine the actual measure of success of tenancy reforms between the two states, given the use of proportions in Karnataka but raw figures for successes in West Bengal. Performance in Karnataka would also be clarified by correction of an apparently misplaced decimal point in Table 4.6 or 4.7, presumably the former. In the case of Uttar Pradish, in Kohli's estimation, the multiparty Janata regime "totally failed" in terms of land reform, thus extending the tradition of its predecessors. This raises the question of whether or not the Janata regime succeeded in other areas of policy in its short two-and-a-half-year span and, if not, whether the two different ministries of severely abbreviated tenure and the anticipation of coalition fragmentation and the probability of new elections did not severely constrain policy initiatives. But given the comparative sweep of the book, the importance of the questions asked, and what has been found, reservations such as these should be seen as refinements for subsequent analyses on a research agenda to which this book makes an important contribution.

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Political Authority and Party Secretaries in Poland, 1975–1986. By Paul G. Lewis. Cambridge: Cambridge University Press, 1989. 344p. $54.50.

This monograph belongs to a genre of inquiry about Leninist states that may best be described as *oblastology,* from the name of an intermediary level of Soviet administration standing between republic and local government. It is the country cousin of *kremlinology* and focuses on the institutional configurations, political mobility, and social characteristics of the regional communist notability. Practitioners of oblastology are distinguishable by their "thick description" of regional political processes; indeed, elaborate research documentation is seen as imperative for two reasons: (1) the powers of this administrative tier are circumscribed by its subordination to higher bodies, hence the concept of power cannot meaningfully form the rationale for research; and (2) findings at this level may throw light on upwardly mobile politicians who, at some future time, may become the focus of analysis for kremlinologists.

Lewis is clearly aware of the limitations to the power held by provincial party leaders in Poland and introduces the more congenial notion of authority to describe the party's efforts to promote "compliance involving consent and some rationally based agreement between rulers and ruled" (p. 7). And while the reader will learn all he or she ever wanted to know—and more—about the functioning of *wojewodztwo* (provincial) party committees (the book thus stands as a masterpiece of *wojewodztwology*), the author also examines the parts played by political and military leaders and the central party apparatus in Poland's dramatic caesuras of the 1980s, from the birth of Solidarity through the emergence of a militarized state to the ineffectiveness of normalization.

The study informs us how administrative and procedural norms—together with the party leaders we already know so much about—helped bring about the demise of Communist party rule in Poland before sister parties were ushered out of power elsewhere in the region. Its meticulous research and finely tuned argumentation alert us to how the Leninist party failed Polish society; and the monograph is a refreshing departure from the more-often-traversed avenue of research—how civil society overcame party rule.

Among the interesting findings recorded here is that the Gierek administration promoted a "carousel of cadres" and "jobs roundabout" at a time when Brezhnev followed his much-publicized policy of "stability of cadres." Lewis points out that this feature of the Polish system did not necessarily contradict—and (we might add) might have served as a corollary to—"the prominence of *nomenklatura* procedures and excessively rigorous control over appointments" during Gierek's rule (p. 40). In addition, the rich data collected on provincial party secretaries shows unequivocally that in fact, Gierek the technocrat appointed fewer provincial leaders who had direct industrial experience. Lewis concludes that such tendencies played some part in the weakening of party authority throughout Polish society. After we learn, further, that under martial law Jaruzelski brought in "an older political generation to replace those elected in 1981" during the Solidarity interlude

(p. 247) and that as late as 1986 there was "little increase in the number of first secretaries with qualifications of direct relevance to industrial and agricultural production or to technology" (p. 293), we begin to see why Polish communists and, with them, the captains of Polish industry and agriculture, increasingly resembled characters caught in the web of a Greek tragedy.

In fact, the author's modesty about the importance of his results may have led him to understate the causal relationship between the developments in the apparatus that he traces and the demise of the Communist party in the 1980s. There are, of course, other reasons for the grand failure of communism; and Lewis is aware that (as one ideologue put it in 1982) the party's "social authority depends less on the ideas it promulgates than on the degree to which they are realized" (p. 215). Thus, research focused on the party's administrative structure cannot do justice to the role played by the dominant ideas of a communist system or the practice based on them. Then again, should scholarship be concerned with such issues when the party leadership itself flaunted its disdain for these mundane matters? The evidence marshaled by Lewis indicates how even the much less ambitious objective of assuring party faithfulness to its own rulemaking—never mind Marxist first principles—often went unrealized. The most notorious case uncovered in this study was the appointment, by the end of 1986, of 19 provincial first secretaries (out of a total of 49) to third terms when party regulations limited office to two terms.

Paraphrasing Jaruzelski, Lewis contends that "the problems surrounding the Polish economy had an 'enormous influence' on the position of the party—but the state of the economy depended 'in large part' on the quality of party work" (p. 297). Although this study is reading primarily for those with an acquired taste for subnational Polish politics, Lewis trenchantly exposes the meager quality of party work not just in Warsaw but in 49 provincial leaderships as well.

RAYMOND TARAS

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Refugees of A Hidden War: The Aftermath of Counterinsurgency in Guatemala. By Beatriz Manz. Albany: State University of New York Press, 1988. 283p. $46.50 cloth, $16.95 paper.

There is a relatively rich literature on the origins, strategies, and tactics of guerrilla insurgencies and government counterinsurgencies. However, there is relatively little documentation or analysis of the effects of (at least temporarily) successful government counterinsurgency campaigns on the social, cultural, and political fabric of a country, particularly on rural areas like those of the Western Highlands of Guatemala.

Manz attempts to assess the lingering effects of counterinsurgency campaigns on refugees returning from Mexico to Huehuetenango (a largely Indian area of the country considered to be a stronghold of support for armed revolutionary organizations); internal refugees "resettled" in so-called model villages and development poles under strict regulation and control by the army; displaced persons hiding in population centers; and Guatemalan refugees still living in official camps in Mexico, which the Guatemalan Army would like to repatriate "en masse." Her study is based primarily on data gathered under difficult and politically precarious conditions through the use of anthropological fieldwork techniques (firsthand observation and extensive interviews with informants) and official documents (from the Guatemalan government, the Guatemalan Army, and international public and private agencies assisting refugees).

Manz argues that the dual war (against armed insurgents and suspected or potential unarmed civilian supporters or sympathizers) was waged, during its period of mass terror (1982–84), "with unprecedented ferocity to eliminate any potential challenge to the status quo" and "went far beyond what was militarily necessary to confront the rebels" (p. 167). The book is mainly about the effects of the army's assault on the unarmed civilian population as part of its attempt to separate the "fish" from the "sea" (i.e., armed guerrillas from their civilian supporters) and create psychological, social, cultural, and economic conditions unfavorable for a recurrence of the political organization and mobilization of whole villages and significant portions of certain ethnic groups (particularly the Ixil and Quiche) that was characteristic of the 1970s.

She concludes that "the military has embedded itself in the countryside in new and far-reaching ways, forcing major cultural adaptations; political constraints continue to prevent fundamental economic and social reform; and the essential guarantees of life and safety are absent" (p. 167).

There is a wealth of information in the book about the background to the current situation, the conditions of refugees in Mexico, international and national laws and policies governing refugees, and various aspects of the government's development poles-model villages program (including maps and charts).

The study also describes the precise mechanisms through which the army has achieved an unprecedented extension of its control over, and influence in, rural areas: the organization of still largely involuntary civil patrols (an institution that not only tends to restrict individual freedoms and mitigate against social cohesion by rewarding villages for spying on each other but also has devastating economic consequences for the cultivation of subsistence crops and seasonal migration for cash wages); ideological and psychological warfare (patriotic ceremonies, denigration of indigenous culture, attempts to blame the guerrillas or refugees for massacres, exacerbation of existing ethnic rivalries); and selected attempts to undermine economic structures supporting traditional culture and social networks.

An important strength of Manz's study, in my opinion, is that it takes seriously the military penetration of the countryside, the trauma left by the massacres, and the continuing obstacles to the safe return of the refugees from Mexico (human rights abuses, lack of access to land, general political uncertainty). At the same time, it does not overlook evidence of resistance to these conditions in the form of humor; passive resistance; periodic abandonment of, and calls for an end to, involuntary civil patrols; and pressure on the civilian president to create conditions that would guarantee the safety of refugees who voluntarily choose to return.

If there is a weakness in the study from the point of view of understanding the interconnections between national and local politics, it is that while national politics are described as part of the setting for the study of local conditions (something relatively uncommon in anthropological studies), the impact on national politics of the economic, political, and social cost of the army's penetration of rural areas is not really addressed. To be fair, this was not the purpose of the study and on balance, even without this assessment, I think it offers an extremely important antidote to the traditional bias in political science analysis which tends to equate "politics" with elections, presidents, Congresses, and national governmental structures. In the case of Guatemala, this bias results in too much attention focused on the election of a civilian president and his rhetoric of reform and too little analysis of the real, continuing powers behind any president—the army and the wealthy elite—and in particular the extent to which both must constantly respond to challenges to their resistance to reform and their access to the means of power.

NORMA STOLTZ CHINCHILLA

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New Politics in Western Europe: The Rise and Success of Green Parties and Alternative Lists. Edited by Ferdinand Muller-Rommel. Boulder: Westview, 1989. 238p. $25.00 paper.

The German Greens: A Social and Political Profile. By Werner Hulsberg. New York: Verso, 1988. 257p. $39.95 cloth, $12.95 paper.

One of the most pronounced political phenomenon of 1989 was that the environment came into its own as a social and political issue. News magazines awarded Earth their status of "person of the year"; when the Western heads of state met in Europe in late spring, their major declaration focused on the environment; tropical rain deforestation seemed to be in the headlines daily; and increasingly global strategists are focusing on the importance of environmental issues for national security.

A partial provocateur in all this has been the phenomenon of the Greens. In the European Parliament elections of late spring the most significant political shift was the more than 100% increase in Green parliamentarians, the election of greens from countries that had never sent a Green to the Euro-Parliament, and the decision of fifteen million Britons to vote for Greens.

The Greens are an occurrence of the 1980s. They are most prominently associated with the success of the West German Green party, though (as these volumes demonstrate) that is an overly narrow perspective. For those who have largely followed the rise and fortunes of the Green movement from international and national papers and magazines, these two books provide an excellent introduction and overview to the process, programmatic structure, composition and possible future of the Western European Greens. Given the rapid changes in Eastern Europe, this literature is especially interesting, as the Greens are a self-conscious attempt to construct a post-Marxist radical social and political movement out of populist citizen's activities; they are seeking to forge a "third way."

As a piece of comparative work, *New Politics in Western Europe* is particularly useful. Muller-Rommel gathered together authors from twelve European nations to write national-level case studies on the Green activities in their respective countries. He charged each author to cover six points (development of the Green party, organizational structure, electoral support, programmatic profile, government participation, and outlook), and chapters are amazingly consistent in the use of this framework. These authors (as well as a chapter on the Green group in the European Parliament) provide valuable accounts of Green activity.

Several things become clear in reading these cases. One is that the greens are a true transnational phenomenon. With very minor exceptions, the goals of the Green parties in the respective countries are strikingly similar. At the same time, *New Politics* also makes clear why the Greens have had varying degrees of success in different countries; the authors highlight the incentives to participation of new political parties.

The thirteen cases in *New Politics* are framed by several chapters that seek cross-country comparison and theoretical perspective. The most interesting issue raised is the extent to which the Greens represent a post-materialist, generational political movement. The question is, Are the Greens the inheritors of 1968? Most authors seem to feel that there is sufficient subjective and empirical evidence to suggest that they are. Survey data of Green party members and supporters intimate that they are younger, better educated, and members of the middle class. Green politics represent a shift toward quality of life and consumerist values. This also implies that there is likely to be tension between Greens and members of the working-class, production-based Left.

New Politics in Western Europe, as an excellent comparative introduction to Western Europe Green activity, is fully complimented by the depth of *The German Greens.* The West German Green party, Die Grunen, is the best-known of the European Green parties. This is partly a result of the strength of its electoral activity, at the state, national and European levels and partly a result of its adeptness at exploiting the art of modern public relations.

In *The German Greens* Hulsberg has prepared an authoritative work on the Greens; it is an astute theoretical treatment of the rise and place of the Greens within German politics. Hulsberg clearly sees the Greens as a type of leftist political party and is able to demonstrate this conclusively. His sympathies are also with them, though he is by no means an uncritical observer. Instead, he seeks to understand the distinction of the Greens from more traditional Marxist parties and theory and speculates whether they represent a truly revolutionary turn in German politics or are instead destined to transform their closest ally, the Social Democratic party.

There are several unique aspects of this book: Hulsberg places the development of the Greens within the broader historical-political development of West Germany; he details the importance of "the new social movements" to the rise of the Greens; and he offers a detailed description of the nuances of the formation and maturation of the Greens, giving the reader ample particulars on the nature of debate among alternative factions within the Greens, as well as the positions of influential individuals. Hulsberg makes clear that the Greens are not a singular phenomenon in West Germany and that the nature of these debates are part of the strength and uniqueness of the party itself. As someone who has tried to follow the Greens largely from the English language press, I found his level of detail especially helpful.

Hulsberg ends his tale of the German Greens in 1987; their success has continued in a dramatic way through 1988 and 1989. As to their future, both Hulsberg and the authors of *New Politics* are quite hopeful. As Thomas Poguntke notes in his comparative discussion of the cases in *New Politics* (p. 194), "As long as the menace of nuclear annihilation is not banned, and as long as one environmental catastrophe is only backstaged by yet another—frequently worse—disaster, it is highly likely that Green parties of various kinds are here to stay."

HARVEY M. JACOBS

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Oltre i vecchi confini: il futuro della sinistra e l'Europa. By Giorgio Napolitano. Milan: Arnoldo Mondadori, 1989. 179p. 23,000 lira.

1992: La sinistra, l'Europe, l'Italia. By Eugenio Peggio. Milan: Sperling & Kupfer, 1989. 240p. 19,500 lira paper.

These books, each authored by a prominent leader of the Italian Communist party (PCI), should be read as companion pieces. From Giorgio Napolitano, long-time member of the PCI directorate and the party's current "shadow foreign minister" we get a declaration of independence from the PCI, as well as an exhortation to Western Europe to seek its own autonomy in a world now freed from bipolar confrontations orchestrated by the two superpowers. Eugenio Peggio, once in charge of the PCI's economic section and the current editor of *Politica e economia*, fills in the details as to the kind of future Europe that is preferred.

These readable volumes provide insights into the thinking of the Miglioristi (Improvers) wing of the PCI. As a reformist group, they are opposed by the PCI Left, and they have been somewhat marginalized by Achille Ochetto, the new PCI general secretary. World events, however, have pushed the new generation of PCI leaders in exactly the direction that these authors, especially Napolitano, have been preaching for many years.

At the risk of oversimplification, their point is that the PCI's future—indeed, its survival—lies in its ability to become integrated in a West European Left, which in turn hinges on the eventual *political* integration of the Continent. Thus, the Single European Act, says Peggio (chap. 3), although a step in the right direction, may be too much of a concession to the likes of Maggie Thatcher. Thus, one may wish to put back on the European Community agenda the Treaty of European Unity advocated by Altiero Spinelli. It's a tall order, dear to European Federationists; and Jean Monnet would marvel at who is placing it.

In the meantime, however, Napolitano not only calls on the Left to make its own contribution to the renewal of a united Europe; he exhorts it to reach for even grander designs. Two of the latter would be (1) disarmament and (2) more effective patterns of cooperation, across arbitrary and now defunct borders that once separated East and West as well as between North and South. For both authors, the topic of the New International Economic Order—with special attention to Third World debt, the overall impact of the U.S. deficit and balance of payments, agricultural subsidies, and problems of the environment—would have high priority.

The New Europe envisaged here would no longer defer to the wishes or (especially) to the unilateral actions of the superpowers. Napolitano notes that the bipolar system is bankrupt not just because the United States and the USSR made errors but, more importantly, because the system stood in the way of other countries' ideas as to how to attack problems. Peggio underscores that Europe's future world role will demand an even greater willingness to forge its own common foreign policies. In any event, echoes Napolitano, jointly tailored solutions put forth by the superpowers should not be accorded privileged consideration. Indeed, Europe will have to be vigilant against any possible U.S.-USSR rapproachment that might be crafted at Europe's expense.

The somewhat Gaullist ring to some of this should not be confused as just more of the PCI's past diffidence toward the United States. Napolitano is quick to observe (p. 139) that "liberty and democracy are *public goods* that can no longer be denied in societies governed for decades in the name of socialism . . . in which there arise irresistible demands for models of democracy such as those that operate in the West." He adds that no opportunity must be spared to help the USSR itself—once the Mecca of all Communist parties—to become a pluralistic democracy based on the government of laws.

The much deeper concern of these authors is that Gorbachev's policies and the revolutionary changes they have triggered in Eastern Europe may be simplistically ballyhooed as a victory of the hardliners against world communism. Even more pernicious would be to interpret these events as proof that the market system, as it has evolved in the United States or (as Mrs. Thatcher would prefer it) in the United Kingdom, should now become the model to be emulated everywhere.

The function of the European Left, of which the PCI intends to be a prominent member, is to preserve the heritage of economic and social gains that distinguish European from U.S. and Japanese conceptions of how the market should operate. The Left must therefore be wary of demands for deregulation; it must be unflinching in its campaign for more equitable distributive policies; it must develop its own strategies regarding unemployment, industrial development and relations, trade relations, antitrust, economic democracy, and a host of other issues and seek to further these within the European Community itself.

Peggio has some biting things to say about Italy's rhetorical support of the European Community juxtaposed with its well-known propensity not to adhere to European Community regulations and directives. He provides (chap. 11) some details as to his country's strength and weakness to meet the challenges of the single European market vaunted by Jacques Delors.

The authors make of European integration the crucible that will test the innovative capacities of a Left that except for some remaining pockets of extremism, is everywhere reformist. It is highly probable that their words will be well received by Willy Brandt, François Mitterand, Neil Kinnock, and other leading European socialists to whom they are obviously directed. Among the latter, however, none is more important than Bettino Craxi, the brilliant and difficult leader of the Italian Socialist party; for it is he who will have the critical say, at least for a time, as to whether the PCI gains membership in the Socialist International. These books are an indication that in that context too, competition for leadership from the Italian Communist party (whatever it may eventually be renamed) will be friendly but formidable.

An unanticipated consequence of the monumental transformations in the East (which no social scientist has predicted) may well be the resurgence of a European Left no longer encumbered by the need to square its policy choices with basic allegiance to the West. The implication here is not that ideology is dead but, rather, that it has been liberated from political and intellectual shibboleths.

JOSEPH LAPALOMBARA

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Between MITI and the Market: Japanese Industrial Policy for High Technology. By Daniel I. Okimoto. Stanford: Stanford University Press, 1989. 267p. $27.50.

According to the U.S. business press, MITI resembles something between a yellow peril and a yellow brick road. The Japanese apparently are doing many things better than the United States, and several types of audiences will read Okimoto's *Between MITI and the Market* with the hope of discovering why. These readers include those who primarily wish to learn about Japan, those most interested in U.S. industrial policy, scholars of innovation and science-technology policy, and those who hope to find transplantable approaches to stimulating commercial high technology. The latter group probably prevails, given the surfeit of books and articles on the applicability of Japan's "miracle" of growth for the United States. Okimoto's book should satisfy readers in each of these audiences.

The book is organized around four chapters and themes: the underlying rationales for Japanese industrial policy, the tools that MITI has applied in the high-technology industry, the recursive relationship between MITI and Japanese industrial organization, and the importance of the ministry's political context. There is no logical starting point among these, so Okimoto frequently assumes too much of his readers, many of whom will be troubled by his tendency (probably unavoidable) to assume that the nuances of Japanese culture and personality are understood already. He also sometimes resorts to generalizations that (valid as they may be) leave the reader feeling that some of the most crucial aspects of Okimoto's topic remain unexplained: "In many contexts and in a variety of industries, the Japanese have demonstrated an unsurpassed talent for collective learning, an ability to organize themselves effectively for such diverse tasks as research, production, and marketing" (p. 86).

Nevertheless, Okimoto is successful in summarizing succinctly the conditions under which Japanese industrial policy tools (administrative guidance or research subsidies, for example) are likely to work. Scholars will find that he weaves together factors in the Japanese economy with which many are familiar (cultural homogeneity and distinctness, freedom from military expenditure burdens, and fluid interest group politics, to mention a few) in suggestive ways. Okimoto is adept at applying Drucker's "market share maximization theory" (p. 41) and Lindbeck's description of "market-conforming methods of intervention" (p. 50) to the dynamic industrial life cycles and the "latecomer" mentality that the Japanese have exploited so well. The book is particularly useful when it refutes popular but mistaken notions about Japanese successes and their causes (such as home market protection) and when it explains how the complexity of the Japanese polity has allowed MITI to develop a variety of "types" of relationships with industry, government, and interest groups that form a "carapace" (p. 225) against partisan political tampering that characterizes, and undermines, industrial policy in other nations.

The book occasionally suffers from lists of seven or eight briefly described "conditions" or "reasons" attached to phenomena under discussion; and a few apparent contradictions appear. For example, Okimoto argues that "in Tokyo, the regulatory philosophy is to push as close as possible to eliminating all risks" (p. 14)—largely because of the relative absence of the courts as arenas of conflict resolution—and that "what makes the Japanese approach so interesting is that it makes government intervention seem less coercive than in the United States, yet it still manages to set regulatory standards that are as high" (pp. 15-16); yet Okimoto also reveals that "the government has not stepped in to provide fuller information on matters vital to the public's health and safety"—such as health warnings about cigarettes (p. 35).

U.S. readers are most likely to compare Okimoto's book with Chalmers Johnson's *MITI and the Japanese Miracle: The Growth of Industrial Policy, 1925-1975* (1982). The two are complements, not competitors; for Johnson focuses more on historical developments and the internal workings of MITI, while Okimoto is more complete on the role of political leaders and parties and on the impact of Japanese industry and its organization on MITI. Some are likely to be disappointed by the book's lack of simple explanations and Japanese magic bullets that can cure U.S. complacency, lethargy, or misdirection. This absence is the source of the book's greatest strength: it forces readers to recognize that Japanese industrial policy has been shaped by a combination of distinctive factors that can inform, but not instruct, other nations' attempts to emulate its success.

RICHARD P. BARKE

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Financial Politics in Contemporary Japan. By Francis McCall Rosenbluth. Ithaca: Cornell University, 1989. 237p. $24.95.

This pioneering study of Japanese political economy analyzes the regulation of finance in the world's largest creditor nation. Banking and financial politics are key to explaining Asian economic success because capital accumulation was the initial task in Japan and East Asia. To explain financial politics, this theoretically informed account asks, Why financial reform in Japan? How does Japan regulate the world's ten largest banks, four large securities companies, a huge postal saving system, and growing international pressures for access to Japanese capital? The answers are not easy, yet Rosenbluth gives elegant insights into how politics matters in Japanese finance.

Studies of public policy and political economy in Japan have moved far beyond simple models of bureaucratic dominance or organizational theory. Most researchers discount the tautology that Japan succeeded because either the bureaucrats or the managers picked winners. Also dissatisfying are answers that Confucian culture made them succeed. In Japan, political scientists argue that institutions matter, that state regulators can monitor market forces, and that public policies encouraged domestic interests to abide by solutions compatible with the market. Finance was a missing piece of the puzzle about Japanese success even though financial politics gave Japanese bureaucrats their initial leverage in the process of economic growth.

Recent research has begun to examine the interests, policymakers, and bargaining that determine regulation of Japanese finance. Comparative studies include that by Louis Pauly's *Opening Financial Markets* (1988), which contrasts the relatively strong influence of Japanese bureaucrats facing international pressures with the influence of U.S. regulators often overwhelmed by domestic banks, with Canadian openness to U.S. and other foreign banks, and with powers of Australian regulators protecting domestic banks. Thomas F. Cargill and Shoichi Royama's *Transition of Finance in Japan and the United States* (1988) reminds readers that despite the mixed effectiveness of financial regulation, regulatory authorities operate with the attitude that they can regulate market forces. Chalmers Johnson and Kent E. Calder have also provided studies showing the importance of financial institutions in Japanese politics and public policy. But until now, financial politics were not adequately analyzed.

Rosenbluth's theoretical framework refines theories of regulation to better account for cross-national variation. Regulation theory based on supply and demand expects that "producers always win and consumers always lose." The theory also predicts cross-national variation only in the types of producers that win. Explaining regulation in Japan requires additional variables. First, state structure based on the constitution and especially bureaucratic jurisdiction provides potent regulatory instruments to the Ministry of Finance as well as the Ministry of Posts and Telecommunications, which oversees the postal saving system. Second, party politics creates a system of one-party dominance whose stability limits any protest or dissent about issues such as consumer finance or banking. Third, industrial organization is dominated by centralization in wholesale finance, although some fragmentation appears among city banks, securities companies, local savings banks, and postal offices. Fourth, international pressure creates an added dimension for past domestic-based theories of regulation, particularly in Japan where import of raw materials and exports of manufactures are crucial to economic growth.

As an authoritative account on the politics of Japanese finance, specialists on Japan will rely on Rosenbluth's findings. Chapters of the study cover the regulatory framework and process of change, foreign pressures, the Banking Act of 1982, the Euromarket where Japanese firms sought funds overseas, and the postal saving system that competes for consumer saving deposits. In addition, the accounts of regulatory politics will be of interest to students of comparative political economy, finance regimes, and international relations.

To assess change in Japanese finance, Rosenbluth shows that regulatory change results from demands of domestic interests more than international pressures. The detailed case studies analyze regulation of interest rates and terms of financial instruments, the issuance of public debt after 1965, the history of the bond market and surplus capital in the 1970s, and the ending of tax-free savings accounts in 1989. On Japan, Rosenbluth goes beyond metaphors that the bureaucrats rule or that a conspiracy reigns. He analyzes conflicts when the Ministry of Finance bureaucrats lose (bank disclosure), win (separating securities and banking), and draw (postal savings).

Readers will not find an account of the implications of Japanese finance as a model of development, an economic locomotive for the world economy, or a stabilizing force in international finance and global order. Such implications get little attention in this book on domestic politics and the theory of regulation; then again, they are obvious. For those lacking background on Japan or for the uninitiated in matters of commercial banking and public finance, the account might be difficult.

Rosenbluth largely overcomes the educational challenges, however, and opens complex issues of finance to the light of political analysis. International finance is too important to ignore and is worth the extra effort by the reader. This book is a needed contribution and gives the best account about how the Japanese case of conflicts in bureaucracy, party, interests, and international context explain the politics of finance and regulation.

STEPHEN J. ANDERSON

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Soviet Central Asia: "A Tragic Experiment." By Boris Z. Rumer. Boston: Unwin Hyman, 1989. 204p. $39.95.

Boris Rumer's book addresses economic problems and their political importance in contemporary Soviet Central Asia. (For the purposes of his study, Rumer defines Soviet Central Asia as the four republics of Uzbekistan, Turkmenistan, Tadjikistan, and Kirghizia.) The author argues that in the coming decades this region may confront Moscow with the USSR's most serious nationality problems. Unlike some other scholars, who tend to see Central Asia's challenge to Moscow in some sort of Islamic fundamentalism, Rumer persuasively argues that over the long run, the center's economic policy plants the seeds for future instability.

Rumer has written a book important not only for Soviet specialists but also for scholars and students who are interested in problems of developing areas. It is well written and free of jargon. The author has based his observations on an intimate knowledge of the Soviet economy and a careful reading of the periodical literature published both in Moscow and Central Asia in the middle of the 1980s.

Rumer introduces his study with a discussion of regional economic policy in the USSR and the role of Central Asia in the Soviet economy. The major part of the book is devoted to the topics of industrialization, cotton, water, labor and employment, and the standard of living. For readers unfamiliar with the USSR, "The Shadow Economy and Organized Crime" may be a surprise, but the author clearly demonstrates why the topic warrants a chapter of its own. Before concluding his book, Rumer considers the likely impact of the Gorbachev reforms on Central Asia.

The most important theme running through the book is the coloniallike relation between Moscow and Central Asia. Rumer illustrates Central Asia's role as a source of raw materials (especially cotton) for the rest of the country. Moscow's investment policies serve to perpetuate the overemphasis of the region's agricultural sector (p. 40). The pursuit of high cotton targets has caused depletion of Central Asia's water resources and destruction of its soil. (Another less obvious byproduct described by Rumer is fraud; Rumer says this resulted when producers found it "virtually impossible to fulfill the plan by honest means" [p. 70].)

Like a colony, Central Asia exports the lion's share of its raw cotton (the major crop) to European areas of the USSR; these sales reflect a low turnover tax added to products obtained from industrial processing of cotton. Meanwhile, allocations for Central Asia's social sphere (e.g., housing, health services, education, recreation, and science) are "exceedingly low" (p. 40). Consequently, Central Asia's depressed living standard shows no sign of improvement relative to other parts of the USSR. Although readers will see parallels with colonial regimes, Rumer argues that Moscow's policy is not merely one designed to "pump out its raw materials and energy resources for the full-scale development of the empire's European center" (p. 61). Rather, the author feels, the situation is much more complex.

Rumer, preferring to avoid many synthetic indexes of Central Asia's economy, offers others that he feels are more suitable in the Soviet context. In a few places, general readers might have benefited from a little more explanation of Rumer's measures. For example, in his illustration of the poor standard of health care, he shows that with only 30.5 doctors per 10,000 population, Central Asia lags far behind the USSR average. However, for readers more familiar with health care in the West than the USSR, this may seem quite high: the "analogous" U.S. indicator is substantially lower than the Central Asian one. A brief explanation of the meaning of Soviet medical data would have cleared things up and convinced the reader that Rumer is on the right track. This is a minor quibble, since Rumer also provides nonstatistical data that support his argument. In the case of health care, for example, he convinces the reader of the correctness of his judgment that particularlay during pregnancy and delivery, the level of health care received by Soviet Central Asian women "is probably indistinguishable from Third World Countries" (p. 139).

Rumer sees little evidence that Moscow is about to change its policies. He documents the center's unwillingness to allocate resources necessary to proceed with the diversion of the Ob River to Central Asia. The region still has the lowest investment share for industry in the country, as well as a low quota for construction. In Rumer's view, although oil, natural gas, and nonferrous metals can be more economically produced outside Central Asia, "surely Central Asia has a clear claim to investment capital to expand its capacities for textile manufacturing" (p. 184).

Rumer's prognosis is grim: he feels that "as long as the production-branch ministries in Moscow control investment and industrial capacity in the national republics, and as long as the republic governments lack the authority to regulate their own economic structure, there is not the slightest chance that the complex, proportional needs of the republic economies will receive greater attention" (p. 165). At least at the time his book went to press, Rumer judged that Gorbachev intended "to preserve and reinforce . . . centralized management of the nation's economy" (p. 167). Other elements of Gorbachev's economic plan cited by Romer (cost accounting and the investment strategy) seem likely to aggravate this.

Closing the book, readers are likely spontaneously to ponder Rumer's concluding words—that the current leadership "simply refuses to come to grips with these problems until a full-blown emergency erupts" (p. 187). This concise volume is a strong argument that such an emergency may be on the horizon.

WILLIAM FIERMAN

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Dilemmas of Social Democracy: The Spanish Socialist Workers Party in the 1980s. By Donald Share. New York: Greenwood, 1989. 169p. $39.95.

The Spanish Socialist Party: A History of Factionalism. By Richard Gillespie. Oxford: Oxford University Press, 1989. 520p. $79.00.

In a country characterized by a history of party fragmentation and political instability, the three consecutive electoral victories of the Spanish Socialist party (PSOE) in 1982, 1986, and 1989 seem a remarkable achievement. The feat is all the more noteworthy for having taken place in a decade that on the whole has not been favorable to the electoral fortunes of the Left in Western Europe. Yet as Richard Gillespie and Donald Share make clear in their highly complementary studies, the political profile and policy initiatives of the PSOE in government have not represented a clear or systematic expression of the socialist or even social-democratic objectives historically advanced by the party. This record of extraordinary political success combined with massive programmatic change is explained in these works in part by reference to internal party politics—especially the political skill, instincts, and resolve of the Sevillian party leaders, Prime Minister Felipe Gonzalez and Vice-Prime Minister Alfonso Guerra. Major aspects of the political and economic context of PSOE activity are also discussed in both works, although they receive much greater attention in the book by Donald Share; the work of Gillespie focuses largely on the historical trajectory of intraparty politics. Both books cover, at some level, virtually the entire history of the PSOE; but their chronological focus is quite different. Gillespie concentrates on the socialists, under dictatorship and democracy, prior to their electoral victory of 1982; whereas Share's analysis is primarily concerned with the socialists in office.

As suggested by his book's subtitle, the bulk of Gillespie's analysis focuses on the internal politics and historical development of the party, especially the frequently bitter factional disputes (in many instances the reflection of deep ideological differences). The result of exhaustive original scholarly research including many interviews with party veterans, the book traces in great detail the development of the party's organization and its thinking. Whereas the period of the republic and the current democratic system have been well researched in excellent scholarly studies, the situation of the PSOE under the Franco regime had been much less thoroughly examined; and therein lies the most important contribution of the book. The ideological conflicts, strategic debates, factional disputes, and personal sacrifices of PSOE members under authoritarian rule are all explored with care. Gillespie is especially concerned to detail the initiatives and political perspectives of specific party members; for the reader interested in following numerous party leaders and activists (some today famous, others little known) through the period of opposition, the book will be a source of important insights and much new information.

This emphasis on intraparty politics leads to many interesting and significant findings, but in places may encourage the reader, if generally uninformed about Spain, to form a somewhat exaggerated impression of the dimensions and impact of the socialists' efforts. This is especially true for the Franco period, when the PSOE was a relatively weak component of the opposition forces. In the same vein, many readers will find somewhat questionable the author's assertion that the party's ideological transformation in recent years "was somewhat more fundamental than the national process" (p. 299), that is, the transition from authoritarianism to democracy. But it would be unfair to dwell excessively on this shortcoming in a book that remains a serious scholarly achievement, contributing in important ways to the literature on the socialists.

In *Dilemmas of Social Democracy* Donald Share clearly and concisely places the political evolution and policy initiatives of the PSOE in the broader context of Spanish political and economic life. Although no specific theme is pursued with the level of detail or the weight of original research findings to be found in *The Spanish Socialist Party,* Share's book is an intellectually engaging and informative contribution. It offers an extremely broad and thoughtful presentation of the initiatives advanced by the socialists and of the constraints they have faced. The attempt throughout the book is to explain the enormous political transformation of the PSOE both before and after the assumption of governmental responsibilities. In a balanced and well-documented analysis Share takes account of several types of causal factors: the difficulties encountered by Socialist and Social Democratic parties throughout Western Europe during the past decade, the distinctive political and economic challenges faced by Spain in the same period, and the specific political opportunities available to PSOE leaders in the 1980s. The result is a book that will prove highly useful to comparativists wishing to learn about the Spanish case and stimulating to students of Spanish politics. Although the coverage of so many themes in a short book offers relatively little opportunity for the subtle or deep exploration of many of the topics covered, the analysis is consistently well presented and persuasive.

Despite the differences between the books, several interesting points of argumentation are found in both. The authors attribute the socialist leadership's ability to quickly and effectively deradicalize the party, in part, to the predominant role in the PSOE of officeholders and government employees who owe their jobs to the party, a predominance accentuated by the very low party membership and by internal decisionmaking mechanisms. Both authors also emphasize the causal significance of the structure of political competition facing the PSOE. The severe crisis of the parties located to the immediate right and left of the socialists, according to this logic, created important incentives and few likely electoral risks for a PSOE move toward the Right. One is left wondering whether and how the behavior of the PSOE government may change in the wake of the 1989 elections, which presented the party with a greatly strengthened challenge from the Left and rejuvenated competition from the Right.

Perhaps the most broadly voiced critique of the PSOE within Spain—complaints concerning the socialists' alleged arrogance in power and their reluctance to acknowledge criticism—is scarcely mentioned in either study. However, the stories told by these authors—especially the internal party developments traced out by Gillespie—are highly relevant on this point. The understanding of this and other dimensions of the Spanish Socialist party has been advanced in important ways by these two complementary books.

ROBERT M. FISHMAN

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Politics of Chaos: The Organizational Causes of Violence in China's Cultural Revolution. By Lynn T. White III. Princeton: Princeton University Press, 1989. 367p. $39.50.

Agrarian Radicalism in China, 1968–1981. By David Zweig. Cambridge: Harvard University Press, 1989. 269p. $30.00.

Western fascination with China's Great Proletarian Cultural Revolution has borne scholarly fruit in at least three waves of analysis. In the first, written in the late 1960s and early 1970s on the basis of Red Guard wall posters and tabloids, the focus was on the power-political machinations leading up to the event and on its political consequences, with greatest emphasis on high-level elite purges and promotions. During the later 1970s and early 1980s, attention turned to an analysis of Red Guard factionalism and on other social ramifications of the conflict, now extending the purview to the 1968–1976 period, and supplementing previous materials with refugee interviews conducted in Hong Kong. Since the mid-1980s there has been an attempt to probe more deeply into underlying causes and consequences of the Cultural Revolution, utilizing the new materials that China's "opening to the outside world" made available. These two books, though different in many respects, are among the finest contributions to this third wave.

Lynn White's *Policies of Chaos* consists of an etiological analysis of the "deep" causes of the initial outburst of violence: only two of its 12 chapters (10 and 11) analyze the "chaos" itself, while nine take us through a careful chronological analysis of the "roots of madness," beginning well before 1949. The focus is on Shanghai, as we have come to expect of this most assiduous of our distinguished cadre of regional microspecialists. The primary source is a massive collection of local newspapers (supplemented by interviews and other later materials), whereby the author hopes to plumb the forces that moved the man (and woman) in the street. But the book goes beyond chronologically arrayed (but delightfully written) "thick description" for a bold macropolitical theoretical statement as well. After classifying, briefly reviewing, and dismissing five previous schools of thought, White introduces his own: the Cultural Revolution arose in response to three "recurrent administrative policies," or "habits of local organization": *labeling groups, designating bosses,* and *legitimating violence in political campaigns.* They were widely (and apparently successfully) practiced throughout the 1949–66 period but drove millions to attack each other when Mao gave license to do so in 1966.

Despite his references to policy, White is in essence resorting to a theory of structural causation, explaining extraordinary developments by tracing them not to an extraordinary causal sequence but to the ordinary—to "standard operating procedures." Patterns of labeling created status groups with built-in antagonisms; building leadership through a web of dependency made underlings follow (or attack) their bosses, and campaigns made such action seem ineffective unless violent. The Cultural Revolution did not alleviate these three patterns but rather exacerbated them, as labeled categories became conflict groups, patronage was at once reviled and promiscuously exploited, and campaigning was conducted nonstop and pell-mell. In the post–Cultural Revolution period, however, White concludes that these standing policies have been modulated. The book provides an important and needed corrective to our usual obsession with elite analysis, unrolling a broader and more vivid canvas to view political developments as they really affect the masses. Of course, as the author recognizes, a structural explanation begs the question of what went awry with the structures; here, the investigation of extraordinary developments, via elite analysis as well as other political science techniques, cannot be dispensed with. It is also unclear to what extent his three problem policies have really been overcome in the post-1978 period. But that is no doubt the topic of White's next book.

David Zweig's *Agrarian Radicalism* contrasts with Lynn White's in a number of respects: while White looks at the urban scene, Zweig looks at the rural; White focuses on causal analysis of the initial outburst, Zweig on later policy developments and long-term consequences. Though Zweig relies heavily on field research conducted in the rural suburbs of Nanjing (Jiangsu province), his analysis is less regionally focused than White's, as he blends a peasant's-eye view with a look at elite policy-making, augmenting interview materials with the content analysis of national media sources, to flesh out a comprehensive picture of agrarian policy during this period—the first we have had since Domes's *China after the Cultural Revolution* (1977), based on far more restricted data.

While Zweig's findings bear out some of our previous assumptions (e.g., concerning the validity of Hong Kong interviews), they also extend them in unexpected ways. The radicals are found to have been more successful in implementing their policies than was previously believed: some 9.5% of all production teams shifted to brigade accounting by 1978, private plots and peasant markets were curtailed, and grain cultivation was vastly expanded. How the radicals promoted their policies, through the use of models, selected purges, "class struggle," and creating "policy winds" through the media is explored in one of the more fascinating sections of the book. Nor did radical policies disappear with the "Gang of Four"; they were pursued (in more moderate form) under Hua Guofeng and his erstwhile agriculture chief, Chen Yunggui. Zweig finds radical policies to have been, on the whole, economically catastrophic; yet a closer reading indicates that good growth rates were sustained during all but the most troubled periods. The rub is that rural standards of living declined badly as a result of a program of forced saving through labor investment. This left the radicals without positive incentives to motivate the peasants, obliging them to rely on the sort of political intimidation that made them unpopular. Rural cadres interposed themselves with considerable success between the radicals at the center and their local peasant constituencies, alleviating draconian policies somewhat; thus, the vast program of rural industrialization seems to have arisen as a compromise between radical demands for greater emphasis on the collective sector and peasant demands for rational investment policies. As far as egalitarianism is concerned, we find that radical policies cut both ways: rural families who had smaller households and more dependents were made worse off by the restriction of private plots, for example; yet the shift from team to brigade level of accounting seems to have accommodated the interests of poor and lower-middle-class peasants. Radicalism seems to have exacerbated rural-urban inequality, while equality within a given county or commune may have been enhanced. Zweig's book is a real landmark, comprising not only a definitive analysis of agrarian radicalism but an indispensable prerequisite for the understanding of subsequent reforms.

The two books are excellent yet so different in focus and approach that it is difficult to compare them. Both agree with the emerging scholarly consensus (after an earlier period of controversy over this point) that the Cultural Revolution was indeed a great misfortune and, moreover, concur on many of the reasons why. Yet the penetration and tenacity of the Maoist ideals is something both have perhaps underestimated, to judge from the most recent of several post-Mao revivals of Maoist rhetoric. Is it conceivable that despite its anarchically populist early incarnation, the basis for the Cultural Revolution's enduring appeal is that it coincides with the interests of *cadres?*

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INTERNATIONAL RELATIONS

The Power of Human Needs in World Society. Edited by Roger A. Coate and Jerel A. Rosati. Boulder, Colorado: Lynn Rienner, 1988. 283p. $35.00.

In the context of a post-1989 world, the arguments that this book makes about the "power of human needs in world society" are difficult to ignore. After the fall of 1989, it is difficult to argue with the statement, "It is the politically realistic observation that, unless there is development and fulfillment of needs of individuals and groups, unless problems are solved and the need for coercion avoided, a social and political order may not be stable and harmonious, no matter what the levels of coercion" (p. 8). This thesis, first stated by John Burton in 1984 (*Global Conflict,* pp. 12–13) is one of the main theses of *The Power of Human Needs in World Society.*

This book is an attempt further to establish the "human needs approach" to international politics. The editors' position is that "world society or world affairs is composed of the interactions of social networks of group relationships based on the individual pursuit of human needs and values" (p. 12). The thesis' clearest formulation is found in the propositions listed in Table 1.1 (p. 13). Central among these are that individuals have enduring needs that are more universal than values or interests, which must be fulfilled and which they will strive to satisfy through group association; for societal harmony and legitimacy, individual needs must be satisfied; and "international relations are not a function of state power politics but of the processes of legitimization and delegitimization due to individual and group pursuit of needs" (p. 13).

After an introductory chapter by the editors describing the "human needs approach" and reviewing its development in the work of Maslow, Sites, Burton, and others, the book is divided into three sections. Part 1 explores the concept of human needs, reprinting classic essays by James C. Davies, John Burton, Ramashray Roy, and Christian Bay. A close reading reveals important differences that might have been more productively examined by the editors in introduction or conclusion: Davies sees wants as needs, the others do not; Burton refers to Sites's work positing eight needs and no hierarchy of needs; Roy argues that calling some needs "basic" devalues others (p. 73); Bay identifies needs as rights, accepting the Maslow hierarchy but finding self-respect a more basic need (pp. 77–78).

Part 2, which explores the application of needs "theory" in a global setting, has two more reprints, by Chadwick Alger and Johan Galtung, and new essays by Rosati, Burton, Craig Murphy, and Coate. The essays by Galtung and Burton are notable for their clear conceptualization (see, e.g., Galtung's Table 7.2, hypothesizing a list of basic needs, on p. 136) and their application to the areas of development and conflict resolution, respectively. These essays, together with some in part 1, verge on theory, though I agree with Galtung that the basic needs approach is not social theory (p. 151). One wonders why the other essays, while interesting, are in this particular book. The linkage to human needs seems peripheral, the analysis in many cases the traditional international relations the editors criticize. Surprisingly, the editors' own articles (Rosati on U.S. foreign policy and Coate on U.S./UNESCO) seem to pay little more than lip-service to the needs-based approach.

Murphy, distinguishing between "basic" and "fundamental" human needs, concludes that global inter-governmental organizations (IGOs) have surprisingly little to do with meeting fundamental needs (p. 212) but a "large role in satisfying basic human needs, in part because they can do so without challenging the international status quo" (p. 216). Murphy recognizes the paradox in the relationship between state power (and thus IGO power) and human needs, as do many of the earlier essays mentioned; but the editors' own contributions, strangely, do not.

This seeming lack of awareness of the fundamentally more complex concept of power required by this approach is perhaps the book's most serious problem. While Sites and Burton posit a more complex conception of power, the book itself overlooks this. Where is the understanding of power that came with the power elite-versus-pluralism debate, with Keohane and Nye's works on transnational relations and on interdependence, and with Gene Sharp's conception of power as based in obedience and consent (*Politics of Nonviolent Action* [1973])?

This lack of attention to politics and power is further echoed in the editor's failure to place this debate in the context of the history of the real-world policy debate on "basic human needs" as an underpinning ideology for U.S. and multilateral foreign aid. In particular, they fail to remind us that this emphasis occurred in a period of declining aid budgets and was widely thought of as a justification for cutting development aid.

Part 3, consisting only of one chapter by Carroll, Rosati, and Coate, in conclusion, is the most disappointing. The questions raised in the introductory chapter (pp. 15–16) and again here (p. 261) give us high hopes, which are never quite met. The reluctance of international relations analysts to pay attention to the concept of human needs is not, in my opinion, due to the "intellectual rigidity that pervades the field of international relations" (p. 260) but to the fact that the approach is not yet sufficiently developed, either theoretically or empirically, at any level, to allow its application to international relations. Nor does this book help a great deal with this problem. The concluding chapter acknowledges "the inability of human needs theorists . . . to build an . . . intellectually coherent paradigm" (p. 260), and its section on "weaknesses and limitations" is convincing. This chapter does retreat a bit from the position that the human needs approach is a replacement for the state power politics paradigm, and suggests that it can be a complement to other existing approaches. This more modest claim is somewhat more credible, and the label *human needs realism* captures this intended link nicely.

The editors' fear that the book may be seen as "far-fetched and idealistic," and the concept of human needs "overly simplistic and idealistic" (p. 218). I do not see this as the problem but rather that this particular edited collection overall does not much advance either the theory or the practice of what has already been identified as a potentially promising approach. This is not to quibble with the fundamental thesis of the book but to argue that the editors have not gone far enough in recognizing the major paradigmatic shift that is needed if one is to adopt this approach. It is puzzling that, while the editors take particular pains to point out that the composition of authors is multidisciplinary and multinational, in fact, all are sociologists or political scientists (or equivalents), and all but one are from Western cultures. One wonders, too, about the fact that all of the authors are male. Would a feminist or female author have provided a different voice? Particularly in an approach where one of the central questions is that of universality versus cultural relativity, one might have expected the editors to seek out a less homogeneous group.

In short, while this book does not seem to be the "landmark book" that Burton proclaims in the foreward, it does contain a number of interesting essays loosely related to the role of human needs in international relations, an area that deserves further conceptualization. If there is a landmark, it is the earlier works of Burton himself. What, then, would a future work have to do? It would need to lay out what needs are and what the epistemology of the area might be: It would lay out hypotheses about universality and hierarchy of needs. It would begin to relate all this to a theory of social change and political behavior at all levels of society. While the book does not provide a great contribution along these lines to international relations theory, it does raise some good questions. One might wish that it made an attempt to answer some of them or at least to specify the research necessary to do so; but even without this, much of it is worth reading and pondering.

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The Waste of Nations: Dysfunctions in the World Economy. By Douglas Dowd. Boulder: Westview, 1989. 138p. $29.95 cloth, $13.95 paper.

The leaking blimp of world economic growth is now snagged on the twin towers of excess capacity and poorly distributed purchasing power. A transformation in the world economy is essential if the airship is to work free and move in the direction of greater prosperity and equality. Writing from a world system perspective, Douglas Dowd chronicles the weaknesses of capitalism and suggests that without major reforms, the world economy might soon undergo a wrenching collapse. His prescription for these economic maladies is to mine the waste inherent in the excesses of capitalist economies and use it to fund wealth redistribution and a host of worthwhile social programs.

The core of Dowd's argument is that nature, current technology, and easily achievable production capacity could provide for the needs of all of the world's 5.3 billion people. This bounty would come from reducing the waste generated by capitalism, which is "the most efficient and the most wasteful production system in history" (p. 64). In support of his argument the author points out that 90% of the retail price of a tube of toothpaste results from marketing costs. He also calls attention to the fact that in 1939 a Chevrolet retailed for $950 but only cost $150 to produce. Maybe he is on to something here; but by broadening his study and updating his automobile data by 50 years, he could have made a much more convincing case.

Dowd identifies several sources of waste in capitalist societies. The problem in industry is waste associated with advertising and planned obsolescence. In the military it's a problem of building new weapons systems that are never used. In agriculture the waste is found in land held out of production in order to keep prices up and adequate food beyond the reach of the bulk of the world's population. Fuel is wasted in transportation because of how people move from place to place.

While the argument sounds reasonable, in some cases the claims do not stand up to close scrutiny. For example, the claim that Texas crude oil costs $15 a barrel to extract while Saudi Arabian crude costs only $2 due to difficult land ownership patterns ignores the fact that differing geological situations and field depletion have a lot to do with costs. In other cases, arguments do not go far enough. For example, while picking on advertising in the United States, Dowd could have broadened his purview and pointed to the waste generated by litigation in a country with nearly one million lawyers.

On balance, Dowd has made a valuable contribution by calling attention to the tremendous amount of "wasted" economic activity that composes GNP in the United States. A major problem, however, lies in casting capitalism as the responsible villain. Surely the recent avalanche of articles about the "wastes of socialism" suggests that this problem is not peculiar to capitalism. On the other hand, the Japanese, while subscribing to capitalist principles, seem to have built a lean and trim economy. Perhaps some other set of variables—such as control of politics and economics by vested interests—could better account for the waste of nations.

The book concludes with suggestions that fundamental changes are needed in the world economy if two different forms of disaster—excess capacity and financial fragility—are to be avoided. But the suggestions offered to avert disaster are hardly novel and are unlikely permanently to solve the problems identified. For example, there is a strong hint that problems of Third World debt could be resolved by refusal of these countries to pay. Left unanswered are the questions of the impact of such action on the fortunes of already-weakened lending institutions and the likelihood that defaulting countries would ever again receive infusions of new capital. A laudable proposal for cutting worldwide military spending by one-third is also offered with the proviso that government nonmilitary spending be increased accordingly. The suggestion for drastic cuts in military spending is not new nor would such cuts be easily accomplished. And in the United States, shoring up unfunded entitlement programs could easily eat into the "peace dividend" without leaving much for other social programs. Finally, the suggestion that cheap food can be produced to feed all of the world's hungry ignores the close relationship between oil prices, modern agriculture, and productivity increases that have taken place mainly in the industrialized countries. The miracles of modern food production are tied to energy-intensive mechanized agriculture; and barring a collapse of world energy prices, it is unlikely that large quantities of cheap food will soon be produced.

Douglas Dowd is on to something when he focuses on waste and the inefficiencies of nations. But many of his prescriptions are unfortunately shop-worn and tainted by his insistence that capitalism is at the root of most evil—in a world characterized by the collapse of socialist countries under the heavy weight of their own military, economic, and political waste.

DENNIS PIRAGES

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The Democratic Imperative: Exporting the American Revolution. By Gregory A. Fossedal. New York: Basic Books, 1989. 293p. $19.95.

Development Aid and Human Rights. By Katarina Tomasevski. New York: St. Martin's Press, 1989. 208p. $39.95.

The Moral Nation: Humanitarianism and U.S. Foreign Policy Today. Edited by Bruce Nichols and Gil Loescher. Notre Dame, IN: University of Notre Dame Press, 1989. 321p. $29.95.

The late Quincy Wright formulated this dilemma: *in the field of foreign policy the U.S.* citizen is often beset by difficulty believing simultaneously in the independence of nations, and the extension of freedom and democracy. But emphasis on noninterference with others results in tolerating oppression, while wishing freedom and human rights on others leads to international tensions. Whether this is a real or only an apparent dilemma is an underlying question pervading the three books under review. The answers supplied range from Gregory A. Fossedal's unabashed exhortation favoring exporting the U.S. Revolution to Katarina Tomasevski's excoriation of the United States' and other aid donors' tendency to use development assistance to punish some populations for the sins of their rulers. Between these respectively jingoistic and iconoclastic perspectives are the multifaceted views represented in Bruce Nichols and Gil Loescher's edited volume devoted to illustrating the difficulties, and reconciling the principles and politics, in the administration of U.S. humanitarian policy.

Each of the three volumes seeks to extend the foreign policy debate over exporting values versus intervention beyond the shibboleths of idealism versus realism, seeking to displace simplisms with careful exposition and analysis of policy. And diverse as they are, each is straightforward in embracing the assumption that international human rights involve global issues that are no longer peripheral to the future of international relations. That this view is shared by a *Wall Street Journal* editorial writer (Fossedal), a European international lawyer (Tomasevski) and the array of voluntary agency activists, social scientists, and philosophers corralled by Nichols and Loescher supports the view that human rights have moved closer to center stage in world affairs and may be ignored or marginalized by U.S. foreign policy only at its peril.

Fossedal's flag-waving treatise celebrates the proposition that the United States is "the first international nation" (p. 232). With evident hubris, the author maneuvers from the Jeffersonian internationalism embedded in the Declaration of Independence to the Lincolnian assertion that "a world half democratic, half totalitarian, is a world that must change in one direction or the other" (p. 238). *The Democratic Imperative* is an extended affirmative answer to the question, Can the United States assist in the conversion of great empires to the principles of our own declaration? The author is at pains to praise the international policies of "men of particular energy—Teddy Roosevelt, Ronald Reagan, Jack Kemp" (p. 238) but laments the "Republican failure to tap the spirit" (p. 229). By the latter, the author makes plain he means the "imperative" to align U.S. foreign policy to what Fossedal perceives as the historical march toward democracy and human rights. To these ends, politicians are exhorted to greater skill in the international use of telecommunications, economic leverage, diplomacy, free trade, and security and development assistance.

Development Aid and Human Rights is a sharply critical primer on the large gap between the declared objectives (including the promotion of human rights) of international development assistance programs and actual practice. The author presents useful case studies (Chile, Ethiopia, Kampuchea, Sri Lanka, and Haiti) showing the uses and abuses of human rights concepts mixed with, and sometimes conflated with, aid. Katarina Tomasevski presents strongly argued briefs on the legal and economic justifications for incorporating human rights in development assistance programs; and in the process, she directs well-aimed critical barbs at U.S., as well as World Bank, policies. The author's thesis is that the purpose of introducing human rights issues into aid programs is to prevent maldevelopment—whose remedy is participation in decisionmaking by those affected. Aid programs should thus seek to promote indigenous, rather than imported, policies; to strengthen grassroots movements; and to encourage popular participation. But bilateral donors (the United States, Canada, Norway, and the Netherlands) have been slow to think through the complex issues of how development aid influences human rights in recipient countries. Most egregiously in the United States, human rights have been treated as an issue separate from development. The U.S. punitive and often arbitrary use of human rights in aid has "overpoliticized the issue," according to the author, and diverted U.S. attention from the merits of a promotional approach to human rights (p. 202).

U.S. foreign policy relative to relief aid for refugees, famine, and human rights victims is the theme of the 13 essays skillfully edited by Nichols and Loescher. Most of the essays in *The Moral Nation* were germinated in the Ethics and Foreign Policy lecture series at the University of Notre Dame (1986–87). The editors define humanitarianism as "the provision of assistance to victims of political or natural disasters" (p. 22). At the outset, philosophers are enlisted to justify such provision of assistance. In a neo-Kantian vein that seeks to invoke universal values, Henry Shue stipulates that such work "must be done"; while Rogers M. Smith uses Lockean terms to frame national aspirations supporting humanitarianism in U.S. foreign policy. The differences in these two approaches are telling; and the constructive disagreements involved set the tone for the remaining essays, in which conflicting opinions serve to clarify, rather than to confuse, complex issues. Useful political analysis is presented regarding the evolution of U.S. governmental and nongovernmental relief agencies (David Forsythe). Perspectives drawn from international humanitarian law shed light on issues of legal protection (Peter Macalister-Smith).

In a lively section on Central America, Doris M. Meissner, Michael McConnell, Gil Loescher, and Lowell W. Livezey analyze the troublesome topics of asylum and sanctuary for refugees from violence-torn areas and assess the roles of private humanitarian organizations in pressing human rights claims with U.S. and regional officials. The volume concludes with accounts by Lawrence A. Pezzullo, Jason E. Clay, Frederick C. Cuny, and Bruce Nichols on the mechanics and some of the poignant disputes involved in recent U.S. famine and refugee assistance in the Horn of Africa.

Diverse as these three volumes are, they are forthright in their moral perspectives and refreshing in their respective avoidance of "see no evil" blinders. Taken together they represent considerable advance over the suppositions articulated by Secretary of State Henry Kissenger, who, in 1975, refused to comply with a congressional request to specify which nations receiving security aid from the U.S. had engaged in repeated "gross violations" of human rights. The secretary said that inasmuch as there was a kind of "original sin" in international relations, the United States should not try to pass judgment on others. The authors of the three volumes reviewed might respond in agreement that there is little to be gained by talking of morality in foreign policy by those who "see no evil."

RICHARD PIERRE CLAUDE

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Reflections on the Cuban Missile Crisis. Revised edition by Raymond L. Garthoff. Washington: Brookings Institution, 1989. 236p. $9.95 paper.

The twenty-fifth anniversary of the Cuban Missile Crisis was the stimulus for the first edition of *Reflections on the Cuban Missile Crisis* (1987). In this new edition, Garthoff offers us an expanded and substantially revised assessment of that crisis based on a number of sources: declassified U.S. documents (many written at the time by the author in his capacity as a staff-level adviser in the State Department), two Harvard conferences in 1987 and 1988, and a third conference in Moscow in 1989 organized by the Soviets, as well as interviews with Soviet officials and writers.

This new information, much of it presented here for the first time, helps to fill in gaps in our knowledge, particularly about Soviet motivations and Soviet decisionmaking. More specifically, the author supplies a dimension of analysis that has been largely neglected: the crisis as experienced by the Soviets and the lessons they appear to have drawn from it.

Garthoff draws attention to the fundamental differences in Soviet and U.S. perceptions of the crisis by underlining the different nomenclature. The Soviets call it "the Caribbean crisis," a reference to U.S. hostility to the Castro regime as a precipitating cause; while the U.S. designation "the Cuban missile crisis," apportions responsibility to the Soviets for their installation of missiles in Cuba. He demonstrates how these differing perceptions resonate throughout the crisis and find expression in conflictual behavior.

Many accounts of the Cuban Missile Crisis view it as having lasted 13 days (16–28 October), from the time Washington discovered that missile construction in Cuba was underway until Khrushchev formally agreed to withdraw the missiles from Cuba and Kennedy pledged not to invade the country. Garthoff places an important emphasis on the period from April to mid-October in exploring Soviet motivations for the missile emplacement and from 28 October to 20 November, in detailing the intensive negotiations between the United States, the USSR, and Cuba that spelled out and modified the settlement that had been reached. His expanded focus alerts us to the value of broadening our perspectives on crises to include both the precrisis and the postcrisis phases.

Readers with a penchant for "insider information" will find a gold mine in *Reflections on the Missile Crisis*. It is replete with wonderful nuggets of information, hitherto largely unknown, based largely on the author's meetings and interviews with Soviet colleagues. Among other things, we learn about the timing of the Soviet decision to send missiles to Cuba, the lack of support for the plan from Khrushchev's senior political advisers, Castro's essentially passive role in the whole affair, the presence in

Cuba of forty-two thousand conventionally armed Soviet combat troops under very senior command, and the probable existence of 20 nuclear warheads in storage under Soviet control. Garthoff also sheds new light on the question of the hardening of terms in the second Khrushchev message and gives us the full story behind the shooting down of the U-2 aircraft over Cuba by a Soviet surface to air missile (SAM), thoroughly debunking earlier speculations in both cases. The content of Garthoff's findings provides a litmus test of why loss of control, particularly in time of crisis, constitutes a major risk of nuclear war.

Garthoff is on somewhat weaker ground in his speculations on the long-term legacy of the missile crisis, as evidenced by his contradictory assessment of the impact of the Cuban Missile Crisis on the subsequent Soviet strategic buildup. Based, in part, on his interviews with General Volkogonov, Garthoff asserts that the Soviet buildup to military parity had been established earlier and was not generated by Soviet failure in the missile crisis (p. 134). Later, however, he argues that the U.S. use of its superior power to compel the withdrawal of Soviet missiles spurred Soviet resolve to acquire countervailing strategic power (p. 145). Finally, he quotes with seeming approval a Soviet source explaining that in the aftermath of the Cuban missile crisis, the Soviet military drew practical conclusions from the United States' clear nuclear superiority and made up for its lag in strategic weapons (pp. 182–83).

In his overall assessment of the Soviet decision to emplace missiles in Cuba, Garthoff asserts that the principal reason was to redress the publicly revealed serious imbalance in the strategic nuclear balance and that Khrushchev favored the missile deployment "to repay the Americans in kind" for surrounding the Soviet Union with military bases and missiles. Garthoff, however, is insufficiently sensitive to his own findings. He acknowledges the value of empathy—the need to see the opponent's perspective—but asks, "Could the United States have reduced Cuban and Soviet fears of an American invasion?" and "Could political means have obviated the perceived need for resort to military measures?" (p. 191). He concludes, rather too pessimistically in my view, that this approach may work for conflict resolution but is less applicable to crisis prevention. A better answer might have focused on the prevention of crises through avoiding the humiliation of one's opponent. The U.S. installation of Jupiter missiles in Turkey, on the Soviet Union's doorstep, and Roswell Gilpatric's public pronouncements in 1961 that the U.S. knew the full extent of Soviet missile inferiority undoubtedly contributed to Soviet feelings of vulnerability and their perceived need to redress the strategic balance. The Cuban Missile Crisis serves as a reminder that failure to empathize with an opponent's needs and fears can, indeed, bring us close to the brink.

BLEMA S. STEINBERG

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Chinese-Soviet Relations, 1937-1945: The Diplomacy of Chinese Nationalism. By John W. Garver. New York: Oxford University Press, 1988. 301p. $34.50.

John Garver has produced an important—and much needed—study. The number of books on Chinese foreign policy remains small, with treatments of pre-1949 foreign policy even rarer. Specifically, it has been a while since a scholar has looked either at Chinese diplomacy during the Second World War or at the wartime relations between the Soviet Union and the Chinese Communist party. With the publication of *Chinese-Soviet Relations, 1937-1949: The Diplomacy of Chinese Nationalism*, earlier scholarship has been both enriched and supplanted.

This is really two books in one. Using the theme of "nationalism," Garver studies the Soviet policy of the two major Chinese politicians of the twentieth century—Chiang Kai-shek and Mao Zedong. Each leader sought to restore China's lost territories and to achieve national sovereignty and international respect. And in each case, a key element for success was the proper management of relations with Moscow. For Chiang, the Soviet Union was a possible source of military aid, a claimant to parts of China, a perceived influence on the Chinese Communist party, and a member of the Grand Alliance. For Mao the challenge was to free himself—and the party he led—from the tutelage of Moscow and the Comintern, whose policies were more concerned with

Soviet national interest than with "Mao's program of national rejuvenation via social revolution." In Garver's view, both leaders did well in achieving their goals vis-à-vis Moscow. "Chiang's wartime diplomatic accomplishments" provided the basis for "China's rise to great-power status," while Mao achieved the emancipation necessary to press for revolutionary victory despite Stalin. In short, at both ends of the political spectrum Chinese nationalism prevailed.

For most specialists, the interpretation and the documentation for the discussion of Guomindang policy will be more interesting but also more controversial. The author's use of a Nationalist compilation of foreign policy documents from Chiang Kai-shek's personal archives as well as of diaries of Guomindang leaders provides new insights into the Sino-Soviet alliance of 1937, the nature of the Sino-Soviet alliance during the very difficult period between the Molotov-Ribbentrop Pact of 1939 and the German invasion of the Soviet Union in June 1941, and the negotiations attending the 1945 Sino-Soviet Treaty. There simply is no other study that does as well with Sino-Soviet relations during these periods.

Of course, while this is the latest word, it is not the last word. Garver relies heavily on data that has been made available by Guomindang authorities that clearly do not represent the full range of diplomatic, archival material that must exist regarding these years. On neither side of the Taiwan Straits does the published new material represent anything more than a very partial—and partisan—record. More substantively, his attempt to enhance Chiang's reputation as a diplomat remains somewhat unconvincing. I would ascribe a much larger share of Chiang's accomplishments to what Garver calls the "fortuitous coincidence between Chiang's objectives" and those of the United States and the Soviet Union than to Chiang's talents as a diplomat. A strategy based on threats to leave the war and often transparent provocations was not the stuff of a brilliant diplomat. Chiang's somewhat Byzantine foreign policy tactics produced much the same results abroad as they did at home: short-term survival but a failure to build the support needed to prevail in the long run.

In recent years Chinese communist materials for internal use (*neibu*) have provided valuable insights into party history and personalities. Of course, they almost always have a political axe to grind and must be used with care. By and large, Garver does exercise such care with these documents; and the new insights into relations with Moscow that result are impressive. However, his discussions of the Chinese Communist party suffer from a failure fully to utilize the party press of these years. A reading of these materials would have provided an important corrective to discussions on specific issues, as well as to Garver's excessive emphasis on anti-Soviet nationalism as a driving force of Maoist policy. Mao was a nationalist; but he was also an inter-nationalist, and the attempt to synthesize these values provided the dynamic for much of his foreign policy. By not giving sufficient attention to both elements, Garver makes a mistake—often serious—in dealing with Mao's worldview: he stresses one side of what was clearly a dialectical relationship.

It must be clear by now that this is a provocative book; and, as with all such books, specialists will disagree. This is not exceptional. But such disagreements should not detract from a general assessment that John Garver, through skillful use of much new documentation, has produced a book that recasts much of the accepted wisdom regarding Sino-Soviet relations during the years 1937–45.

STEVEN M. GOLDSTEIN

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Studies of War. By Henk Houweling and Jan G. Siccama. Dordrecht, The Netherlands: Martinus Nijhoff, 1988. 258p. $75.00.

The use of the plural in the title of this book renders it a quite accurate description of the volume's contents; for it contains a variety of theoretical, empirical, and historical analyses of war rather than a single integrated study. This rather eclectic approach may be considered by some a weakness of the book, but what the volume lacks in coherence is more than compensated for by the breadth of topics covered.

The first part of the book contains several theoretical essays covering a variety of subjects. One deals with the basic beliefs and attitudes that make war possible, another with the inherent tendencies for conflict to escalate to ever-higher levels, and a third with the question of whether war is primarily an inherent, inevitable outcome of the interaction of sovereign states or as a result of conscious, largely rational, calculations of policymakers. The last essay in this section of the book discusses the research methodology that has been used by the Correlates of War Project, a research tradition within which the authors explicitly state they are operating (indeed, the book is dedicated to J. David Singer). The first three essays in this part of the book, although interesting, contain relatively little that is new for those who have made a serious study of war; but the last will certainly interest those who are involved in its empirical study. The authors' discussion of the various criticisms the project has received over the years is particularly worthy of note.

The central portion of the book contains five quantitative studies of war dealing with a number of important questions. Does earlier war promote subsequent war? Do nations differ significantly in their war-proneness? Is war contagious across time or space? Does the existence of an arms race between two nations make war between them more likely? Is war more likely between two states if one is overtaking the other in terms of material capability? To address these questions, the authors rely upon the Correlates of War data and employ some innovative techniques; and the answers they obtain can only be crudely summarized here. When international and civil wars are combined, there is evidence that war promotes war; states differ radically in their war-proneness; in some regions of the world, war appears to have had an epidemiclike quality; arms races, by themselves, do not seem to make war more likely; and power transitions do appear to make war more likely. Most of the work reported in this section of the book has been previously published in various journals, although the statement is made that these are extended versions of those earlier studies. In any event, they are first-class empirical efforts and make a significant contribution to our slowly growing empirical knowledge of war.

The final section of the book contains two exercises in diplomatic history. The first focuses on what the authors refer to as the self-destruction of Europe and the United States and Soviet Union's overtaking it, while the second discusses the implications of the relative erosion of U.S. power and its loss of hegemonic dominance in the international system. Together, these offer some interesting insights into past historical developments and possible future trends.

The somewhat disjointed nature of this review reflects the equally disjointed nature of the book; for fundamentally, the book is a collection of interesting but unrelated papers. This is not a criticism of the work in and of itself; for almost without exception, the empirical study of war has been marked by a lack of integration. Most of the work in this area consists of rather narrowly focused studies distributed across a dozen journals and several scores of edited volumes. Infrequent efforts to put the findings of these studies together or develop more comprehensive analyses have largely been unsatisfactory. Hence, the authors of this book can not truly be faulted for failing to do what others have not been able to do either. It is enough, I think, that they have made some significant contributions to our understanding of war in this work.

STUART A. BREMER

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The Meaning of the Nuclear Revolution: Statecraft and the Prospect of Armageddon. By Robert Jervis. Ithaca, NY: Cornell University Press, 1989. 266p. $21.95.

Since the days of Thucydides, it has been a central insight of the realist approach to international relations that sovereign states face a security dilemma. Efforts to make oneself more secure by building military strength could simultaneously make an adversary feel less secure. Now, in the nuclear age, some statesmen and analysts argue that the only path to national security is mutual security.

Robert Jervis argues that nuclear weapons have dramatically altered the nature of statecraft. The enormous destructiveness of nuclear weapons has severed the connection between war and political objectives for nuclear armed states. In 1918 and 1945, "the winners still were better off in having fought the war than they would have been had they made the concessions necessary to avoid it" (p. 7). But mutual vulnerability to nuclear annihilation means that even the winner is not better off after the war than before it. The result is that force cannot support foreign policy as it did in the past. The United States and the Soviet Union "have fate-control over the other, but it is far from clear how much this can translate into behavior control" (p. 3). The consequence is an international system marked by unprecedented peace, preservation of the status quo, and the infrequency of crises.

The destructive force of nuclear weapons is not new. Jervis cites Bernard Brodie's 1946 book, *The Absolute Weapon,* and quotes President Eisenhower's view that "there is no victory except through our imaginations" (p. 4). Why, then, did it seem a breakthrough when Reagan and Gorbachev declared at their 1985 summit that a nuclear war cannot be won and must never be fought? Jervis blames the fallacy of "conventionalization"—of treating nuclear weapons in the same terms as war-fighting conventional weapons. The temptation to think in such terms is reinforced by the stability-instability paradox. Strategic stability creates instability by making lower levels of violence safe and undermining guarantees to allies. But Jervis argues that the paradox is not as stark as often portrayed and that elaborate nuclear counterforce strategies are not needed for extended deterrence.

Those who have read Jervis's 1984 book, *The Illogic of American Nuclear Strategy,* will find the general arguments familiar but better put. It was always odd that Jervis, a connoisseur of psychological and domestic political factors, had previously argued his case in terms of unitary states (and only one side, at that). The new book restores the richness of texture of his argument, though more for the U.S. than the Soviet actor. (Jervis does not try to discuss the recent changes in Soviet domestic and foreign policy.)

The weakest chapter in the book, however, is the treatment of ethics. By failing to discuss different types of counterforce strategies (such as countercombatant rather than countersilo targeting), Jervis avoids discussion of some possibilities. His meager conclusions about prudence are partly a result of the dichotomy of *counter-force* and *counter-city* that he uses to frame his discussion.

In short, this book is a convincing, well-written argument that nicely critiques strategy while summarizing the consequences of the nuclear revolution for world politics.

JOSEPH S. NYE, JR.

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Third World Military Expenditure: Determinants and Implications. By Robert McKinlay. New York: Pinter, 1989. 154p. $39.00.

Robert McKinlay's *Third World Military Expenditure: Determinants and Implications* is a useful, though uninspired, addition to the expanding body of literature on the political economy of defense in the developing world. The empirical focus here is on post–World War II (1950–82, actually) military spending by Third World countries. The reader is shown, for instance, how the Third World's share of total world military expenditures has increased over time and how military expenditures have increased faster than gross domestic products (GDPs). Yet the variation in absolute and relative levels and rates of growth of military spending is stressed throughout the book. McKinlay notes that despite the priority apparently attached to military spending, military expenditures were not increasing in about one-fifth of the countries being examined and were actually declining in another one-fifth.

Despite the wealth of empirical data presented in the book, McKinlay's primary objective is more broadly theoretical: "to identify the determinants of Third World military expenditure and to combine these determinants into a theory of military expenditure" (p. 12). This theory is to provide an explanatory account of the ubiquitous and enduring but variable nature of military spending in the Third World. The author's secondary objective is to examine the implications of defense spending.

McKinlay's cross-sectional analysis (which provides only a series of snapshots of specific points in time rather than a sustained historical analysis) of the determinants of military spending yields a number of interesting observations. His analysis of the relationship between a range of absolute power capabilities and military spending reveals that GDP is the "single best predictor of military expenditure" (p. 23). An examination of the relationship between military expenditure and budgetary behavior leads the author to observe that "countries have larger budgets because they have larger military expenditure[s] rather than they have larger military expenditures because they have larger budgets" (p. 33); that military spending is unconstrained by short- or long-term financial constraints such as relative public debt and interest payments; and that military spending has greater autonomy than health and education expenditures but does not have a negative impact on education and health expenditures.

After unnecessarily trudging through possible "realist" and "idealist" perspectives on international interactions in a chapter on the empirical relationship between military expenditures and international interactions, McKinlay concludes that military spending is essentially independent from private sector international interactions such as trade and investment, while public sector international interactions such as diplomatic ties and arms imports drive military spending upward. An additional conclusion that the diffusion of capitalism cannot be regarded as having stimulated military spending, while interesting, is not based on any empirical evidence presented in the book.

Trenchant criticisms of bureaucratic politics' explanations of military spending are offered in the context of an investigation of the relationship between domestic conflict and military expenditures. But the author's analysis of the relationship between military influence on the state (as measured by military coups, military regimes, and the proportion of executive governmental positions occupied by military representatives) and military spending is flawed by the explicit omission of attempted coups and the failure to factor into the analysis of precise executive positions—and the relative importance of the executive positions—occupied by the military; for where the military sits may well influence where the government stands. While the author finds that there is no evidence to suggest that direct military influence affects military spending (indeed, there is no discernable difference in the levels of defense spending by civilian and military regimes) or that sustained, organized domestic violence contributes only marginally to higher levels of military spending, armed interstate conflict, especially at higher levels, is found to be a powerful stimulant.

Many of McKinlay's empirical findings will provoke debate and spur additional research—a worthy accomplishment. Yet after having labored through six dense empirical chapters of this slim book, the reader is bound to be somewhat surprised (dismayed?) to find that the author's explanation of the ubiquitous and enduring nature of the Third World's commitment to military spending rests not on his extensive empirical analysis but on a rather brief (pp. 132-36) discussion of five assumptions drawn from the realist literature on international relations. McKinlay's empirical results, which he neglects to integrate into a coherent whole, are intended to "explain" only the variability of military spending. Theory, again, has given way to quantitative empirical description. A more inspired and insightful analysis of the determinants and implications of military spending in the developing world can be found in Nicole Ball's *Security and Economy in the Third World* [reviewed above—ED] (1988).

Andrew L. Ross

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The Onset of World War. By Manus I. Midlarsky. Boston: Unwin Hyman, 1988. 268p. $39.95.

Midlarsky uses hierarchical equilibria theory to account for the initiation and diffusion of global war and then proposes justifications for actions that could minimize such encounters. *Systemic war* is a restrictive concept applied only to conflicts "entailing the breakdown of the international system as it existed prior to the outbreak of the war" (p. 3). Systemic war is manifested as either (1) a structural encounter motivated by prior conflict among several protagonists and enhanced by a large-scale international crisis (e.g., the Peloponnesian War of 431-404 B.C., the Thirty Years' War of 1618-48, the French Revolutionary Wars of 1792-1802, and World War I of 1914-18) or (2) a mobilization conflict whose onset is triggered by domestic mobilization (e.g., the Macedonian War of 357-336 B.C., the War of Spanish Succession of 1702-13, the Napoleonic Wars of 1803-1815, and World War II of 1939-1945) (pp. 12-14). Systemic wars differ from *general wars*, also fought on a grand scale, because the latter do not fundamentally disrupt belief systems within nation-states or restructure international arrangements (e.g., the War of the Austrian Succession of 1740–48, and the Seven Years' War of 1756–63). Midlarsky's objective is, then, to explain systemic war, a rare event (recorded in Western history less than 10 times) that like a massive earthquake, shakes the inner workings of the participating nation-states and leaves lasting scars on the international system.

Midlarsky's research suggests that a systemic war can be averted when the international system is oriented to power parity, the great powers maintain hierarchical relationships toward smaller powers, and a general fluidity of alliance partners is maintained. On the other hand, global war is likely when hierarchy breaks down and resource inequality permeates international arrangements. These results are both supportive and critical of previous attempts to relate great wars to power distributions and alliance structures. Midlarsky suggests that alternate models based on power distributions or alliances are only partially successful, because they consolidate different types of systemic war under a single rubric, and pay insufficient attention to the relationship between hierarchy, power distribution, and internal mobilization. From his perspective understanding structural wars necessitates concurrent analysis of structural changes in the international distribution of power and alliances structures; and for understanding mobilization wars, a consideration of domestic factors that can independently precipitate the onset of war must be added to this systemic evaluation.

Midlarsky's work is a broad examination of great wars that goes beyond the evaluation of the onset of conflict. For example, a particularly innovative application of the polya distribution is used to explore the rate of diffusion of systemic war and show that expansion differs across wars but can be anticipated. Also, in an unusual but very innovative application, John Rawls's principles of justice *A Theory of Justice,* (1971) are used to justify the partitions of Poland in the eighteenth century by connecting these partitions to the absence of systemic war during that period.

The analysis of systemic war, like others on this subject, is constrained by the very small universe of great wars available for analysis; but Midlarsky still manages to use quite innovative formal techniques in parts of the study to show that this type of war follows systematic and analytically predictable patterns. At the same time, Midlarsky skillfully employs case studies and effective historical narrative where such techniques are the appropriate vehicle to make a given point. The main drawback is that while each portion of the complex theoretical structure proposed is investigated separately, all the components are not brought together to demonstrate the integrity of the whole structure.

Such a limitation is perhaps inevitable in this kind of work since the number of wars to be explained once the universe is partitioned is so small that alternative views cannot be easily rejected. Moreover, despite the very extensive work on past wars, there is perhaps too little concern with the impact of these results on the nuclear period. Such comments are not meant to detract from the importance of this complete, challenging, extensively documented, and extremely well researched book. Midlarsky has no doubt concentrated on rare, systemic wars because, like many predecessors, he wishes to understand the rationale behind massive changes in the international arena and gain a foothold on controlling these events. Given the difficulty of the subject treated and the high objectives set by the author, one must stand in awe of how much he accomplishes.

This important contribution will no doubt become required reading for all advanced undergraduate courses on war. It is a must read for graduate students and all scholars with serious research interest in war.

JACEK KUGLER

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Retreat from Doomsday: The Obsolescence of Major War. By John Mueller. New York: Basic Books, 1989. 327p. $20.95.

This book is built around two profoundly optimistic central points: (1) war is not a necessary consequence of social organization but a matter of human choice; and (2) in today's world, as well as the foreseeable future, a central war among major powers is highly unlikely because (as the author puts it) that kind of war has become obsolete. Mueller's case does not present new data but rather hinges on his interpretation of history since World War I. Prior to that war, it was fairly easy to find otherwise intelligent individuals advancing arguments on behalf of war; and the obvious anachronism of these arguments is testimony to Mueller's case. War was variously described as noble, manly, or glorious; while peace was often described as corrupt or decadent. The carnage of World War I, however, made war "rationally unthinkable." Civilization lost its taste for war, just has it had earlier lost its taste for dueling and slavery.

But how can this argument be sustained in the face of World War II and the Cold War? The former led to more casualties than World War I, and the latter holds the possibility of nuclear war. Recognizing this difficulty, Mueller devotes a considerable portion of his analysis to these two events. Mueller argues that World War II was almost entirely a product of the personal policies of Adolf Hitler.

A major portion of the book is occupied with Mueller's explanation of how major power war can be obsolete while both the United States and the Soviet Union have pursued policies predicated upon the threat of such an event. Mueller argues that nuclear weapons are irrelevant because (1) Soviet goals are much more limited than Western political leaders had thought and (2) given these limited goals, the threat of a war on the order of World War II has been sufficient to deter Soviet leaders. Although he admits that nuclear weapons might serve as an "insurance policy" (p. 219) against escalation, Mueller's central point is that a major war in Europe, even in the terms of World War II, has become so transparently irrational that the "long peace" seems likely to continue. Moreover, communist ideology is unmistakably mellowing, first in the Soviet Union and now perhaps in China, whose leaders at one time made a sport of castigating Khrushchev for his advocacy of peaceful coexistence.

There is much to be said on behalf of this book. Its central topic is unquestionably important, the reasoning and evidence are usually carefully drawn, and the conclusions are optimistic without being utopian. On top of this, it is uncommonly well written and manages at the same time to be both lively and deadly serious.

Despite these very considerable merits, there are a few things that may bother some readers. Occasionally, the analysis comes very close to wishful thinking. For example, Mueller argues that a conventional war in Europe would not necessarily escalate to a nuclear exchange because decisionmakers would be aware of the costs of such an exchange and would seek to deter each other from that escalation (pp. 236–40). The problem, of course, is that such a possibility rests on the assumption that decisionmakers know what is going on, a possibility that is questioned just a few pages earlier (p. 228).

One might also question the analysis of World War II. To be sure, Hitler was risk-acceptant in the extreme and was certainly evil; but it is by no means clear that he wanted what eventually became World War II. This is not an apology for his role in the war; but it is reasonably certain that Hitler thought he could get what he wanted in small, quick wars, perhaps even using the horror of World War I as a tool against the British and French. In the end, however, the British and French were reluctantly willing to take him on despite the costs they thought possible. Put simply, there were political goals for which nations would wage a major war. They may well still exist.

More importantly, the analysis of the book would undoubtedly be more satisfying to some readers if there were a theoretical explanation for the absence of war that did not rest so heavily on the claim that people's tastes have changed. Lacking a theory to which the analysis may be tied, we are left with the hope that the recent (i.e., post-1945) situation will continue and that people like Hitler will continue to be missing from the leadership of the major powers. Unfortunately, we are also left with the realization that human affairs do not usually run in a straight line. There have been long periods of peace before; and all too often they gave way to war when the system could not accommodate the demands of nations who sought to change the international order to something more to their liking.

RANDOLPH M. SIVERSON

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Global Technopolitics: The International Politics of Technology and Resources. By Dennis Pirages. Pacific Grove, CA: Brooks/Cole, 1989. 220p. $17.00 paper.

We could use a new text for courses on world politics, one that examines more than states and their interests and that discusses problems and opportunities at the individual and global levels as well as in terms of states and regimes. The premise of this text by Dennis Pirages is one I agree with, namely, that international relations are changing fundamentally and that new approaches are needed in order to understand them. Pirages calls the approach he takes "inclusionist," that is, a perspective that considers "humankind as one among many species competing for the earth's limited resources . . . [and] is oriented to analysis of change" (p. 8). His model features three sets of variables: *technoecological* variables like the physical environment of the planet, population and other resources, and technology; *structural* variables consisting of political, economic, and social institutions and patterns; and *values* variables, that is, ideology, philosophy, and religion. So far, so good; but when we examine the causal linkages between and among these sets of variables, we can see one of the reasons why this book does not live up to the promise of its premise.

Pirages sees these three sets of variables as hierarchically organized: the technoecological variables affect structural variables, which in turn affect values. Causality is one-way—there are no feedback loops (p. 7). Pirages explicitly identifies the causes of change as ecological and technological, in contrast to "the stabilizing and guiding role played by existing values and institutions" (p. 9), which are seen as outcomes of change or constraints on change rather than as agents in their own right.

Most of the book describes the world situation in a "global resources" framework that examines population, energy, food, air, water, minerals, and technology. Interestingly, given Pirage's criticism of short-term perspectives, philosophical materialism, realist ideology, and U.S. politics, his analysis is chronistic, materialistic, liberal in the nineteenth-century sense (the basis of realism as a political ideology), and centered on the United States and its place in the world. Thus, despite its promising beginning, the book turns out to be a standard exposition of global problems by a U.S. scholar taking a limits-to-growth point of view.

Pirages did not look for a more complex interplay among the variables, such as a possible connection between technology and elite values. This is a common problem of many recent works on technology and automation, which assume that technology follows its own imperative while the rest of us just struggle to keep up. The notion that technology might involve fundamental political choices—such as who shall have agency—is rarely explored. But you need not be a Luddite to resist technological determinism, especially when it might be screening evidence of political behavior.

Pirages also takes a rather unimaginative perspective on the impact of scale. His discussion of population is conducted almost entirely in terms of geophysical carrying capacity, seen in modern times as extended by technology, which enables human beings to utilize capitalized resources. Yet transformations of social organization arising from changes in scale are probably not linear, as Pirage's discussion of lateral pressure implies they are. What happens to democracy when citizens live in communities larger than Periclean Athens or Plymouth, Vermont? Even though individuals may still exercise choice in the political arena, republics are not democracies. An analogy in the economic sphere would be the loss of worker autonomy and consumer sovereignty that occurs when the face-to-face economic relations of Adam Smith are replaced by production and distribution systems dominated by a few powerful actors. For all the pieties reflected in nineteenth-century disquisitions on substantive due process, the power of the worker is not equivalent to the power of the manager plus the power of the state.

The world is changing and we need a new text that reflects the possibilities as well as the inevitabilities of the new age, a *Twenty-Years' Crisis* or a *Great Transformation* that is embedded in the physical, and the political, universe as we perceive it today. Unfortunately, despite its tantalizing first chapter, *Global Technopolitics* is not that book.

MARY ANN TETREAULT

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Wars of the Third Kind: Conflict in Underdeveloped Countries. By Edward E. Rice. Berkeley: University of California Press, 1988. 186p. $18.95.

Edward E. Rice's book is a critical examination of guerrilla wars and counterinsurgency policies. Writing for policymakers and the general public, the author warns major powers of the pitfalls of intervening in Third World conflicts. While Rice addresses a broad audience, political scientists could also benefit from his analysis.

Rice analyzes the origins and evolution of wars of insurgency and critiques the policies adopted by major powers as they become embroiled in these wars. This type of conflict, found most often in Third World countries, is what Rice refers to as "wars of the third kind." In recent years, military interest in low-intensity warfare has increased in the wake of Vietnam and in the face of ongoing conflict in Central America. Rice argues persuasively that many of the "lessons" learned from Vietnam are, unfortunately, the wrong lessons and that major powers have much to lose by becoming involved in such wars.

This is a well-written contribution to an important subject. Rice presents a lucid argument drawing from his extensive personal experience as a U.S. foreign service officer and as a deputy assistant secretary of state. He utilizes an extensive set of cases to develop and substantiate his points, ranging from the U.S. War of Independence and the Napoleonic Peninsular War to the Chinese Civil War, the Huk Rebellion in the Philippines, various conflicts in Central America, and the Algerian and Vietnam Wars.

The book breaks down into two parts. The first part of this work focuses on the origin and evolution of insurgency movements. The second part examines various counterinsurgency tactics and the potential problems facing major powers involved in these "wars of the third kind."

In his discussion of the origin and development of guerrilla movements, Rice identifies three phases to wars of insurgency. The first involves the organization and preparation of the movement. Rice concludes that such rebellions essentially stem from popular uprisings against the repression or tyranny of an indigenous or foreign government. Rice further contends that "it is in the rural areas of the underdeveloped lands of the so-called Third World, and only in such lands, that wars of third kind take place" (p. 52). His contention is that rebels require an extensive outback with dispersed sources of support to conduct guerrilla operations. While Rice refers to a few cases of failed urban guerrilla movements (e.g., Uruguay's Tupamaro National Liberation Front), he does not adequately distinguish these "wars of a third kind" from similar types of movements in Western Europe, e.g., the Irish Republican Army. In general, Rice's work would have been strengthened had he more completely developed his analysis of the origins of Third World guerrilla movements.

Rice provides some interesting new insights in his examination of the evolution of guerrilla movements into the second and third phases of insurgency. His discussion of how guerrilla forces are transformed into "regular armies" is particularly fascinating and is illustrative of the major strength of this work. He observes that as these groups develop, both their organizational requirements and their military tactics change. Rice's analysis integrates these two concerns, showing how both are vital for understanding how insurgencies grow and develop.

The second part of this work is a policy prescription for major powers that may become involved in such conflicts. In particular, counterinsurgency tactics are compared, and the costs of foreign intervention are evaluated. This is the heart of the book, and it is evident that this is what really interests Rice. His discussion of counterinsurgency is an excellent overview for a general audience. Rice strikes a nice balance by writing at a level understandable to beginners and meaty enough for the more advanced reader.

This book is a very readable account of "wars of the third kind." Rice's work suffers from a few minor problems, however. Some points could have been strengthened through better-developed summaries of the arguments. A more explicit and systematic comparison of various cases would also have helped. Despite these limitations, the book successfully addresses a complex topic in an approachable manner.

SCOTT GATES

Michigan State University

Environmental Cooperation between the North Sea States. By Sunneva Saetevik. New York: Columbia University Press, 1988. 173p. $35.00.

This study is an updated version of a University of Oslo doctoral thesis (p. xiii). It was carried out in 1985–86 as part of a research project on new regimes for the management of oceans at the Fridtjof Nansen Institute of Norway, which is known for its high-quality work. Sunneva Saetevik draws from extensive interviews with secretariat members and delegates to the Oslo and Paris commissions that were established to implement the Oslo Convention for the Prevention of Marine Pollution by Dumping from Ships and Aircraft and the Paris Convention for the Prevention of Marine Pollution from Hand and Sources, both of which came into force in 1978. Clearly, it is an authoritative work on an important topic. While it suffers somewhat from the constraints of dissertation writing, Saetevik's work is readable. Her somewhat labored English would have benefited from more substantial editing, especially in the concluding chapter.

The work is heavily, though usefully, methodological. Chapter 3, "A Framework for Analysis" presents an analytical model operationalized (I use the author's terms) in the next six chapters. The final chapter suggests a revised model for determining policy outcomes. Although the members of the Oslo and Paris commissions have agreed that they have a common interest in protecting the marine environment of the North Sea and that a regional management regime is the way to promote it, they have not agreed on regulatory measures. This is due to the fact that the decisionmaking process depends on states that retain their sovereign independence (even though the majority of them belong to the European Community). Indeed, the European Community itself is a player in the game of North Sea environmental management.

The study will interest two classes of readers: those who are concerned about the global environment as professionals or interested laypersons and those who as researchers or teachers are anxious to know how to design, conduct, and evaluate research. I have successfully used this short study, for example, to help graduate students researching topics other than North Sea pollution.

For those interested in the possibilities of combating pollution through intergovernmental cooperation in a well-defined region, the study provides little ground for optimism. Even in chapter 5, "Policy Outcomes," little is revealed about any actual success in combating pollution since the establishment of the Paris Commission. The author examines policy outcomes as binding decisions, recommendations, and nondecisions for pollutants such as mercury and oil-based muds. The study informs both the layperson and the professional why these outcomes appear so meager in a region where multilateralism might be expected to be effective. In the author's paradigm the policy outcome is the dependent variable (chapter 3), and the perceptions and ability of member states to promote their own preferences and institutional constraints are the independent variables. While this explanatory paradigm may seem labored to the lay reader, it is worthwhile for the policymaker and the academic. All concerned may be startled to learn that the role of the European Community in the policy process has been a principal factor in limiting the policy outcomes. This leads to "the paradox that policy outcomes of the Paris commission may, in reality, be determined by states who do not participate in the cooperation" (p. 30). Most decisions are in fact recommendations (chapter 10)—which require only a majority vote, whereas binding decisions require consensus. Regulatory policy thus appears to be shaped by the lowest common denominator. Because the European Commission is one of the actors and its preference is also determined by consensus, the lowest becomes very low indeed. Saetevik reassures us to some extent by noting that results are not fairly measured by outcomes limited to "decisions" and "recommendations." She implies that what member governments actually do to deal with pollution is a better measure and that their actions are determined in part by the political and information-sharing process in the commissions established by the two conventions.

The work, in sum, is an excellent first cut at understanding the problems of environmental cooperation in the North Sea region. A follow-up study is needed to see whether or not pollution is increasing or decreasing in the area.

DANIEL CHEEVER

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Moving Targets: Nuclear Strategy and National Security. By Scott D. Sagan. Princeton: Princeton University Press, 1989. 235p. $19.95.

Many believe that the major theoretical works on nuclear strategy date from the 1950s and early 1960s; since then, this view holds, nuclear academics have elaborated, but not much improved on, these early works. I do not fully share that opinion—for example, Graham T. Allison's *Essence of Decision* (1971) on the Cuban Missile Crisis ranks with the finest—but it often seems true that works on nuclear strategy have tilled and retilled the same patch to exhaustion.

Recently, however, some genuinely provocative and enlightening work has been done by younger specialists exploring the operational and decisionmaking approaches pioneered by Allison. In addition, historians are now working with once-classified U.S. materials from the 1940s, 1950s, and much of the 1960s, now released through the Freedom of Information Act (FOIA), the presidential libraries, direct interviewing, and other routes. (Similar materials for other countries are much scarcer, although non-American writers like Lawrence Freedman and Desmond Bell have made important contributions on the United States.) This work, in aggregate, is providing a new look at the history of U.S. nuclear policymaking by incorporating, as well, the development of U.S. nuclear operational policy. We have already had several full-length surveys based largely on contemporary public material and interviews (e.g., Lawrence Freedman's *Evolution of Nuclear Strategy* [1981], Fred Kaplan's *Wizards of Armageddon* [1983], and Gregg Herkens's *Counsels of War,* [1985]), as well as a number of articles in journals and collections using FOIA material. Other efforts are ongoing, such as the Nuclear History Program (headed by Ernest May of Harvard but based at the University of Maryland), which is coordinating work in the United States and Europe. Finally, as in Sagan's new book, this historical material has begun to enrich the examination of current operational issues.

Sagan's book is not a comprehensive review of U.S. nuclear policy, strategic deterrence, or strategic force issues. It assumes its readers have at least a general familiarity with these basics. Instead, it comprises a discussion of selected topics in U.S. nuclear policy: nuclear doctrine and its evolution; the current state of nuclear operational planning, along with some proposed changes; limited strategic defense options, along with criticisms of possible roles for such defenses; accidents and warning time issues, along with some proposed improvements; and a final set of comments and conclusions.

Sagan's approach is one that is both familiar to, and comfortable for, a Washington reader. He tries to understand the reasons for existing policies, examines their successes and failures in achieving their intended objectives and suggests possible modifications of those policies either to better meet their objectives or to satisfy adopted goals. This approach essentially works at the margin, with practical feasibility and balancing of competing considerations always in focus. It is far from a comprehensive factual review of even the issues it addresses; it by no means replaces, for example, the massive Ashton B. Carter, John D. Steinbruner, and Charles A. Zraket's Brookings edited collection on nuclear operations, *Managing Nuclear Operations* (1987). Nevertheless, Sagan has produced a *useful* book, containing reasonable analyses and sensible judgments; even where one disagrees with them, they are lucid, fair, and stimulating.

Of particular interest are the chapters describing the development of U.S. targeting doctrine and more critically discussing and evaluating current doctrine. Sagan generally approves of the current variant of "countervailing" targeting doctrine, while suggesting a number of nuanced modifications. He perhaps falls a little short in not more fully describing some of the implications of what he calls the "usability paradox"—the disjunction between the need to avoid nuclear war and the felt requirement to make nuclear deterrence effective—particularly in not emphasizing the centrality of the alliance dimension in the history of U.S. nuclear doctrine. As Lawrence Freedman has emphasized, the task of bringing strategic nuclear deterrence to bear on deterrence in Europe without destabilizing the superpower deterrence equation has, in various forms, been at the heart of most of the key U.S. strategic nuclear policy problems of the last four decades.

Sagan's chapter on accidents and warning is a sound review, although suffering somewhat from its not discussing the new U.S.-Soviet Agreement on the Prevention of Dangerous Military Activities (signed in June 1989, presumably after the book went to press), which addresses a number of Sagan's concerns. Sagan does note the potential of what he terms "operational arms control"; but since many of his examples of dangerous incidents date from the 1950s and 1960s, he might have summarized intervening U.S.-Soviet agreements in this area, especially the 1971 Accidents Agreement and the 1972 Incidents at Sea Agreement, with its annual consultations thereafter.

The general approach of the book could be criticized for its emphasis on marginal changes to policy and for what it does not attempt. The book has no examination of the bases of deterrence or of basic principles of stability; it does not address force structure issues or their relation to deterrence; like most other works in the strategic policy field, it retains an essentially bipolar perspective; and it does not consider moral or philosophical issues. It does not, in summary, consider challenges to, rather than improvements on, the existing strategic policy structure. Such an effort seems needed in the extraordinary world setting at the end of 1989, though it will also be essential to consider the strategic requirements that will remain and the new ones that will arise. Perhaps Sagan's next work will take this on.

ROBERT LUCAS FISCHER

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Israel and Conventional Deterrence: Border Warfare from 1953 to 1970. By Jonathan Shimshoni. Ithaca: Cornell University Press, 1988. 247p. $32.50.

The protracted and multifaceted Arab-Israeli dispute produced over the years a welter of academic and journalistic works. Most analyses focused on events of great salience and magnitude, such as the Six Day War of June 1967 and the October War of 1973. Depicted as the screen through which the conflict could be elucidated and explained, these wars became the main points of departure for studying a plethora of issues; and little attention was paid to a variety of low-level and incremental types of violence that could not match the dramatic impact of major conflagrations.

In this respect, *Israel and Conventional Deterrence* makes a valuable contribution to the literature. Exploring the dynamics of border warfare as they unfolded in three distinct sequences between 1953 and 1970, the author establishes the missing linkage between the manifest and the latent, or the assertive and the subtle. By viewing the outbreak of full-scale rounds of war between Israel and its neighbors as the culmination of these sequences of sublimated conflict rather than as disjointed and isolated phases, he provides a nuanced and balanced perspective to this research field, one that is sensitive to the potential of escalation and deterioration inherent in continuous, unabated border skirmishes.

Shimshoni's work is patterned closely on the premises of the analytic-inductive approach to theory development, which treats individual case studies as divergent building blocks for the construction of a typological, highly differentiated, and contextual deterrence theory. Recognizing that the deductive and paradigmatic logic of nuclear deterrence on the superpower level is seldom appropriate for analyzing the considerably more complex, fluid, and ambiguous context within which conventional deterrence takes place, he adopts a relativistic criterion for assessing deterrence success, one confined to a specific level of action and to a limited time frame (pp. 15, 26, 28, 33, 235).

In applying his analytical-inductive research design to the Arab-Israeli conflict, Shimshoni juxtaposes three episodes in which Israel apparently pursued a deterrence strategy vis-à-vis Jordan and Egypt. These include (1) *fedayeen* (guerrilla) infiltration from Jordan, 1953–54; (2) harassment from Egypt, 1953–56, including *fedayeen* infiltration from the Gaza Strip and direct Egyptian harassment in the form of military violence, 1954–55; and (3) The Egyptian-Israeli War of Attrition, 1967–70 (p. 33).

Although the relativistic, context-dependent approach a priori precludes any sweeping and definitive conclusions regarding the long-term success or failure of deterrence, Shimshoni's findings do suggest a clear dichotomy between the Israeli-Jordanian dyad of 1953–54, and the Israeli-Egyptian dyad of 1953–56 in terms of the immediate ramifications of the strategy on both fronts. (The findings of the third case study are less conclusive.)

For all its analytical insights and sensitivity to the regional context within which various forms of deterrence were implemented from 1953 to 1970, *Israel and Conventional Deterrence* suffers from several deficiencies and shortcomings—in terms of structure, methodology, and content—that, altogether, considerably weaken its analysis and argumentation.

In the first place, although all three case studies incorporate components of both general and immediate deterrence, none can be thought of as a pure and unmitigated illustration of either variant of the phenomenon. By virtue of their sequential nature, these episodes surpass the delimited and constrained parameters within which immediate deterrence is attempted. On the other hand, being illustrative of only one level of military activity, they do not even approximate the degree of aggregation and comprehensiveness that the concept of general deterrence entails.

Furthermore, it can be argued that rather than seeking to prevent another party from doing something it has not yet done, most—albeit not all—Israeli leaders between 1953 and 1956 pursued *compellence*, namely, a posture designed to stop Jordan and Egypt from doing something they were already in the process of doing (directly or indirectly). Unfortunately, this reductionist propensity to explain diverse and heterogeneous processes in terms of a single analytical concept (refined and multifaceted as it might be) results in bias and occasionally unfounded interpretations. Because of preoccupation with the notion of deterrence, an entire cluster of factors and variables remains largely obfuscated and unexplored. For example, in examining the strategy of reprisals as it unfolded between 1953 and 1956, the author does not adequately address a host of domestic considerations, constraints, and calculations (such as the perceived need of the Israeli leadership to raise the morale of the embattled border settlements by resorting to a strategy of reprisals or the vision held by many high-ranking officers in the Israeli Defense Forces, according to which the continuous engagement of the army in retaliatory raids and reprisals constituted the optimal way for improving its level of professional performance), which are completely decoupled from any deterrence designs. Worse still, irrevocably committed to a fixed set of preconceived notions, according to which the objective of Israeli reprisals (between 1954 and 1956) "was to deter Egypt from sending Fedayeen and from instigating direct military clashes with Israeli forces" (p. 38), the author ignores the fact that at least one central Israeli decisionmaker (Chief of Staff Dayan) strongly advocated the strategy of reprisals not as a means of deterring (or compelling) Egypt and Jordan but rather as the necessary provocation or catalyst for a large-scale preventive war.

For all these weaknesses and shortcomings, *Israel and Conventional Deterrence* stands as a thoughtful effort to unveil the vastly complex dynamics and manifestations of deterrence. Its partial failure to accomplish this mission should by no means deter other scholars from embarking on a similar—albeit not identical—analytical and empirical research course.

ABRAHAM BEN-ZVI

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Democratizing Brazil: Problems of Transition and Consolidation. Edited by Alfred Stepan. New York: Oxford University Press, 1989. 404p. $35.00 cloth, $14.95 paper.

Once again, Alfred Stepan has gathered an impressive collection of essays at an important juncture in Brazilian history in order to shed some light on where Brazil has been recently and where it might be going. As I write, two candidates face off in the final debate prior to the deciding round of Brazil's first direct presidential elections since 1960. As these essays show, the Brazil the winner will confront is quite different from the Brazil of thirty years ago.

Stepan has captured the essence of the moment in the book's title, which he discusses in an introductory essay. At the time of publication (1973) of this volume's predecessor, *Authoritarian Brazil*, Brazil was in the midst of an extended authoritarian experiment; in 1989, although formally democratic, Brazil may be more accurately be described as *democratizing*.

The twenty-one years of military government (1964-85) witnessed profound changes in Brazilian society, some positive, others negative: the emergence of a sizable middle class, diversification and deepening of the economy, and concentration of wealth, to name a few. Francisco Weffort, in the last part of the volume (p. 345), captures an underlying theme of the book when he observes: "More than the 'economic miracle,' the terror years [of the late 1960s and early 1970s] produced a real 'political miracle,' by undermining traditional ideas on the relations between State and society." Stepan and his collaborators analyze the interplay of these and other changes with the extended liberalization/democratization project of 1974–84 from a variety of angles.

By way of background, historian Thomas Skidmore recounts how the military moved from arbitrary rule to the rule of law. Those familiar with contemporary Brazilian history will find no surprises here, but the chapter is useful as a general introduction to the period under discussion. Bolivar Lamounier, one of Brazil's foremost researchers on voting behavior, follows with a brilliant analysis of the impact of the regime's desire to retain democratic legitimacy through promoting elections (despite opposition at times from within) and the opposition's use of those same elections to deny the legitimacy of the regime, force it to acknowledge defeat, and return power to civilians. In doing so, Lamounier addresses one of the acknowledged shortcomings of the earlier volume: inattention to purely political factors that eventually played a major role in Brazil's transition.

The second part of the book addresses the external constraints faced by Brazil during the 1970s and 1980s. Albert Fishlow's thorough discussion of economic policy decisions and the environment in which they were made, although soundly based in economic theory, says disappointingly little about the impact of economics on politics in general and the redemocratization of the country in particular. Edmar Bacha and Pedro Malan draw lessons from the mistakes of the 1980s and address the dangers the debt question presents for the future.

The third part of the book deals with democratization of society, as opposed to democratization of the regime. Ralph Della Cava provides an outstanding discussion of how the Brazilian church became one of the strongest opponents of the regime during the years of repression and how a conservative restoration within the church at home and abroad has now taken it largely out of politics. Unfortunately, the narration of alternating progressive and conservative ascendancies in the Brazilian church obscures the cohesion it achieved during the years of repression. Scott Mainwaring adds a discussion of the interplay of grassroots organizations and democratization at the local level. Sonia Alvarez' somewhat rambling essay on women's participation in Brazil's redemocratization discusses how women fought to have their concerns addressed by local and national governments rather than their important role in the broader prodemocracy movement. Margaret Keck completes the section with an excellent discussion of the union movement's growth, diversification, and internal conflicts. The unions have been among the strongest advocates of change in Brazilian society in recent years, and Keck highlights the strategies adopted by competing factions within the labor movement.

In Part Four, Fernando Henrique Cardoso reconfirms his earlier notion of dependent development before analyzing how the state has become all-pervasive in Brazil, why large sectors of society have rejected state interventionism, and what shape democratic Brazilian society might take. This essay and the following one, by Francisco Weffort, are particularly germane to the current debate in Brazil over the nature of, and relationship between, the state and society. Weffort is particularly insightful in examining fundamental questions regarding democracy in Brazil: how it is perceived by various sectors of society, why it exists, and what must be done to complete the democratization process.

Finally, Maria do Carmo Campello de Souza discusses the period immediately following the return to civilian government and notes a number of continuities that run up against changes in civil society, such as the inability of traditional clientelism to cope with an expanded franchise. She emphasizes the incompleteness of the transition, the danger of high expectations, and the question of sustainability of democracy, once achieved, in Brazil.

J. Ray Kennedy

Library of Congress

Book Reviews: International Relations

Perspectives on Deterrence. Edited by Paul C. Stern, Robert Axelrod, Robert Jervis, and Ray Radner. New York: Oxford University Press, 1989. 343p. $42.00 cloth, $19.95 paper.

Defending Deterrence: Managing the ABM Treaty Regime into the Twenty-First Century. Edited by Antonia Handler Chayes and Paul Doty. McLean, VA: Pergamon-Brassey's, 1989. 286p. $32.00.

Although these two volumes share the word *deterrence* in their titles, the similarity ends there. The Stern and colleagues volume is a series of essays that attempt to grasp the essence of strategic deterrence by looking at the concept from a variety of disciplinary perspectives. The Chayes and Doty volume, on the other hand, consists of a set of analyses about how to ensure that the Antiballistic Missile (ABM) Treaty is not successfully assaulted in the 1990s.

Perspectives on Deterrence deals with a familiar theme, the attempt to gain some measure of empirical understanding of how deterrence works. Since the empirical base on which deterrence rests is almost entirely devoid of empirical content and, further, since more detailed empirical evidence can only be adduced through the failure of deterrence (i.e., nuclear war), the quest must be carried out by looking for parallels outside the central concern of U.S.-Soviet nuclear balance and dynamics. To attempt this inquiry, the editors have brought together a number of disciplinary and substantive approaches. These range from "detailed historical analysis" to psychological approaches to analyses using quantitative and game-theoretical constructs. At the same time, the deterrent relationship is viewed through the substantive lenses of oligopolistic competition, international trade, rebellions and revolutions, the "aggressive male" syndrome, and interpersonal versus international aggressive behavior. The editors cite the wide variety of contexts in which deterrence is examined as the volume's unique contribution to our understanding.

This is not so much new ground as fallow ground reworked. The fact that deterrence is both the subject of investigation and the value to be maintained has always meant that the central problem—avoidance of superpower nuclear war—has had to be approached by indirection; and finding an isomorphic case has always proven difficult, even elusive. The frustration that absence of an obvious analogue creates come through in this volume, particularly since it has as an implicit theme the notion that the failure of deterrence is an ever-present possibility, if not likelihood. The particular essays in this volume are both interesting and generally well done, but it is hard to see how they very much advance our understanding of the central concern that activated the study.

The topic of *Defending Deterrence* is the ABM Treaty and what steps are necessary to maintain its viability for the balance of the century and beyond. Although the assumption is neither raised nor explicitly defended in any detail, the wisdom of continuing and strengthening the treaty is implicit throughout. The basis of this underlying rationale is the success of the treaty in limiting a form of the nuclear arms competition, the contribution of the ABM Treaty to general strategic stability, the negative arms control connotations of its weakening or abrogation, and the chilling effect such action would have on East-West relations. These arguments do not form the core of any of the articles in the book or the totality of the work, but they underly the reason the book exists.

At heart, the studies that comprise *Defending Deterrence* appear to have two underlying purposes. The first, and by far most prominent, is to examine sources of potential weakening of the conditions in which the ABM Treaty "regime" resides and to suggest how strengthening can be guaranteed. The examinations tend to be highly technical and—to the reader lacking a reasonably sophisticated understanding of physical sciences (especially physics)—arcane in their detail and prescription. Those not written from a scientific perspective are legal and legalistic. The second purpose is rather clearly to provide rationales and strategies to block implementation of elements of President Reagan's Strategic Defense Initiative (SDI). Given the apparent policy shift downgrading the priority of SDI by the Bush administration, this latter concern seems somewhat less compelling.

This is not a book for the general reader. None of the authors, for instance, is a trained political scientist; although several carry the title professor of public policy. Rather, all were trained either as physical scientists or, in a few instances, lawyers; and their work reflects those backgrounds. The real value of this work is as a source book for someone negotiating or planning strategy to ensure continuation and strengthening of the ABM Treaty. Given the events of 1989 in East-West relations, the AMB Treaty seems in a great deal less jeopardy than it did several years ago. This book would have been much more timely then.

DONALD M. SNOW

U.S. Army War College

Escape from Violence: Conflict and the Refugee Crisis in the Developing World. By Aristide Zolberg, Astri Suhrke, and Sergio Aguayo. New York: Oxford University Press, 1989. 380p. $49.95.

For the past three decades the great majority of the world's recognized refugees have appeared in the Third World, where they present exceedingly difficult moral, legal, political, and economic challenges to the international community. With a few exceptions, refugee outflows have been perceived as singular and unpredictable occurrences and as primarily humanitarian rather than political problems. The central theme of *Escape from Violence* is that refugee movements in the Third World are not produced by random upheavals and conflicts but are the result of identifiable, patterned processes related to social conflict and the forms that state- and nation-building efforts are taking under current conditions, which vary from region to region. Similarly, the policy responses to refugee flows form patterns that are conditioned in part by the nature of the initial conflict that produced refugees as well as by the characteristics of the flow and the disposition of the potential receivers. Moreover, the book demonstrates that refugee flows must properly be understood in a transnational perspective because (1) the social conflict that produces refugees is shaped by international forces, (2) external intervention in a conflict often exacerbates the refugee situation, and (3) the exit and admissions policies of states directly influence refugee flows. Accordingly, *Escape from Violence* is primarily concerned with the political dimensions of the refugee phenomenon—how refugees come about, how states respond to them, and how refugee groups can become political and military actors on the national or international level.

Until recently, the refugee issue attracted only the limited attention of political scientists. There have been virtually no studies in the social science literature on refugee movements treating causes and outcomes of refugee flows in a systematic comparative fashion, and there has long been the need to integrate refugee studies with broader social science literature on conflict, development, and foreign policy analysis. Drawing on the literature that deals with ethnic, societal, and interstate conflict (as well as several detailed, descriptive studies of particular refugee movements) and employing a historical-sociological method of analysis, the authors of *Escape from Violence* provide a theoretical structure of the varied causes of refugee movements in the Third World and contribute to the generation of a systematic body of data on refugees that increases our understanding of the principal factors inducing their movement. This is a matter of some significance, since it is impossible to determine satisfactory solutions to the global refugee problem without a good knowledge of the various causes and determinants of refugee flows.

The authors conclude by arguing that the fear that instability and oppression in the Third World will give rise to a sustained (and possibly increased) flow of refugees in the future is unwarranted and that some of the historical processes that created the contemporary global refugee crisis have been played out. While not wanting to promote a doomsday perspective, I believe that there is little to suggest that these numbers will decrease significantly in the near future, particularly in the poorer areas of the world. Population growth, economic stagnation and underdevelopment, political instability, increased militarism and the ever-growing arms trade, and seemingly intractable ethnic conflicts all signal the persistance of sizable forced population movements in the future. The likelihood that refugees in the Third World will be endemic in the years to come does not, of course, mean that there also will not be other refugee crises elsewhere. The outflows at the end of the 1980s of Poles; East Germans; ethnic Germans from the Soviet Union, Poland, and Romania; Jews and Armenians from the Soviet Union; ethnic Hungarians from Romania; and ethnic Turks from Bulgaria—all remind us that the periodic eruptions that have occurred in Eastern and Central Europe in the last generation are not yet finished.

Moreover, almost every war or ethnic or religious conflict that generates streams of refugees also creates a massive problem of internal displacement. Those who have been uprooted by war and human rights violations but have not left their home countries are not considered in this study, but they are perhaps in even greater need of humanitarian aid and protection. In future years, therefore, we should expect refugees and displaced people to continue to appear in considerable numbers all over the world. However, by identifying the patterns of social conflict that are characteristic of refugee-generating regions of the Third World, the authors of *Escape from Violence* enable us both to better understand the nature of refugees and to clarify what international responses might be most appropriate to resolving the global refugee problem.

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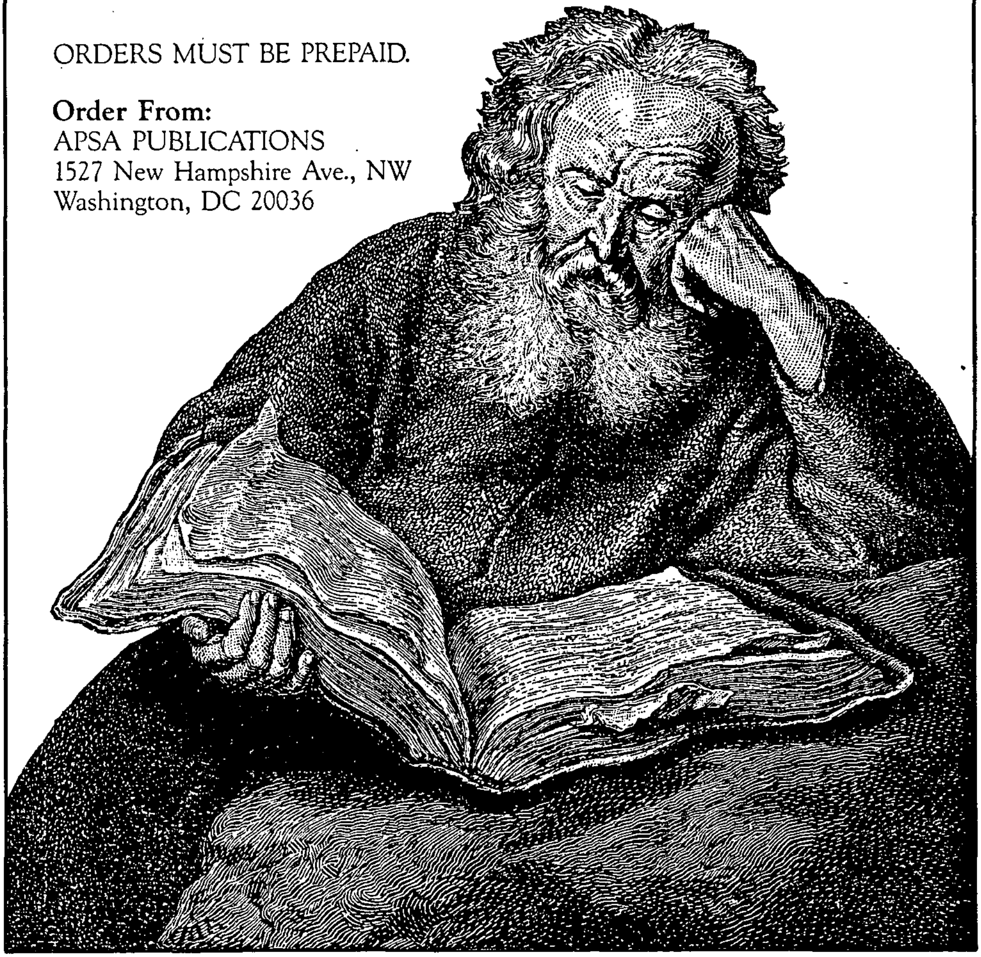

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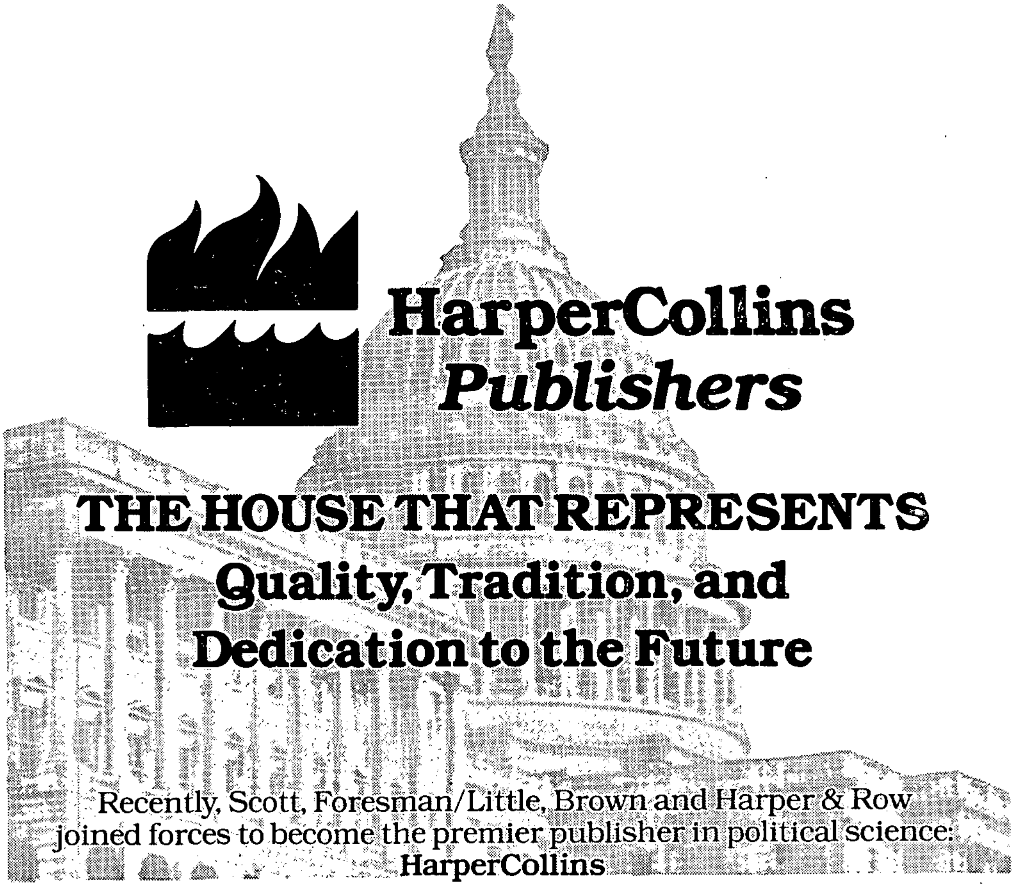

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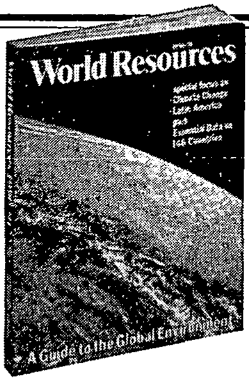

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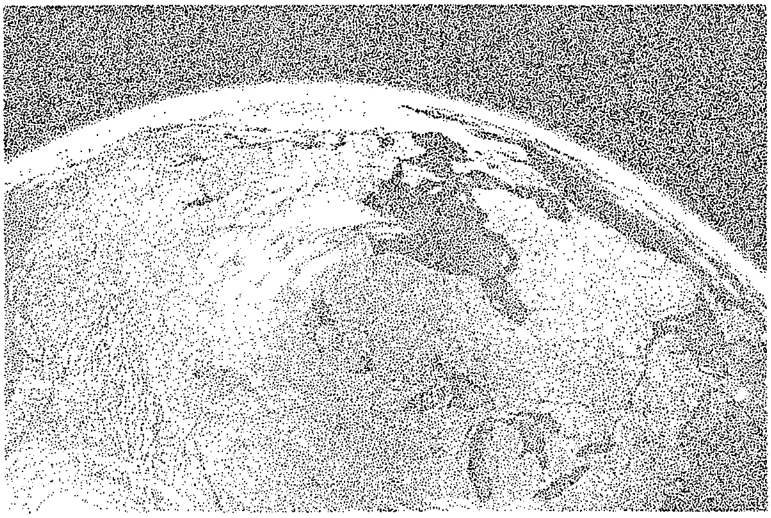

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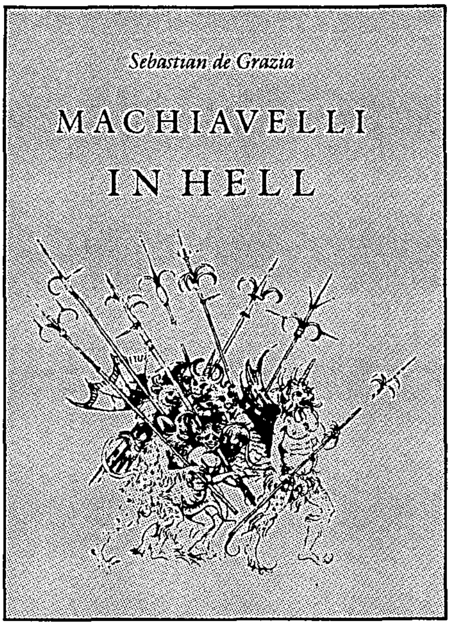

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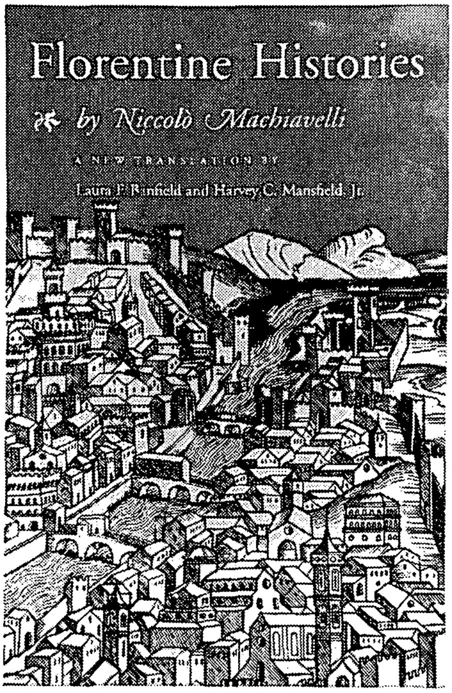

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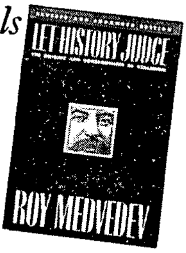

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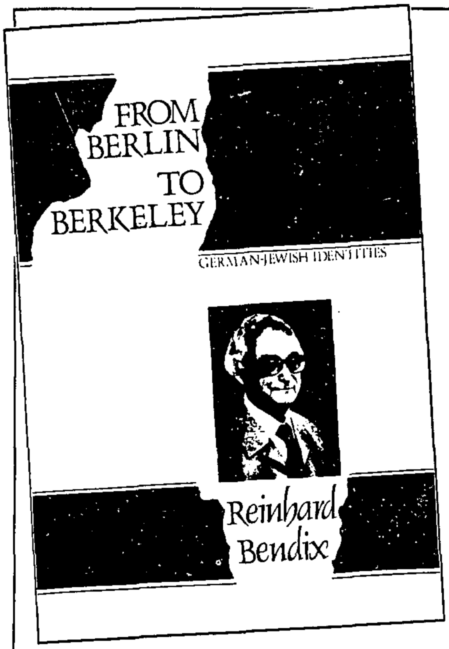

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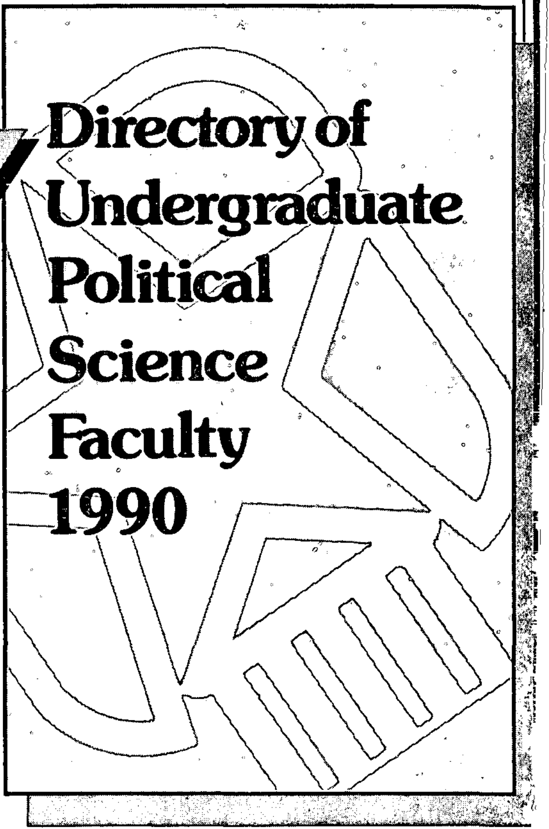

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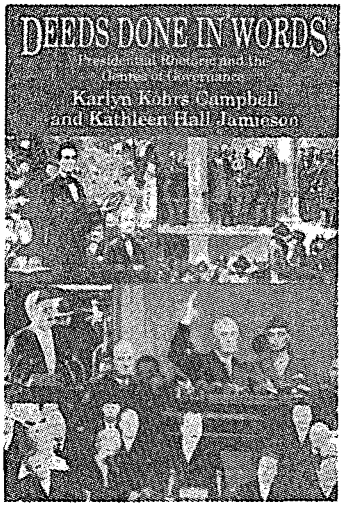
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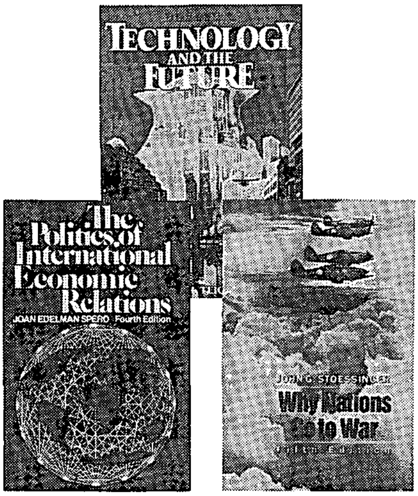
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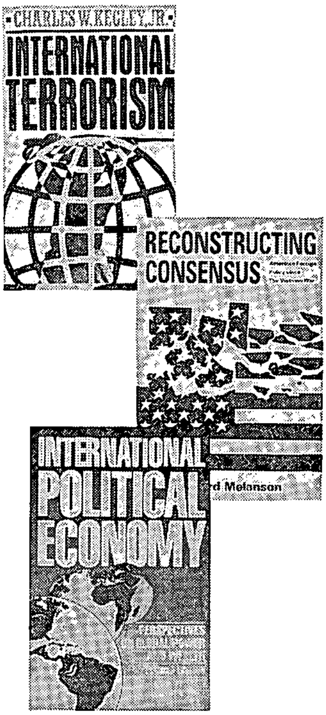

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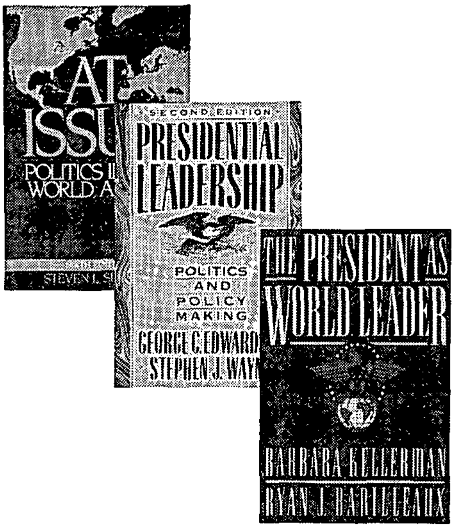

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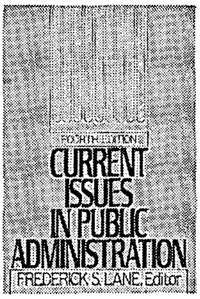
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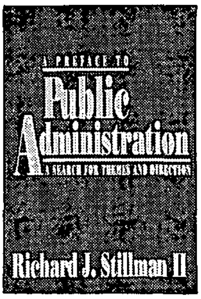

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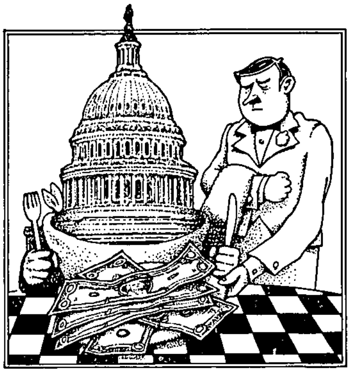

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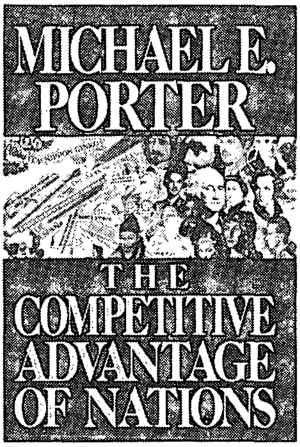

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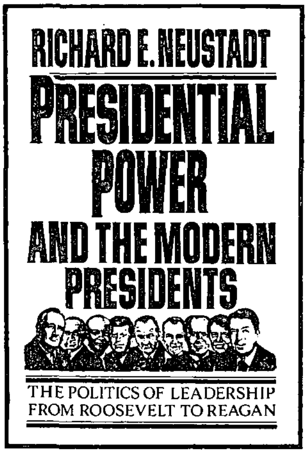
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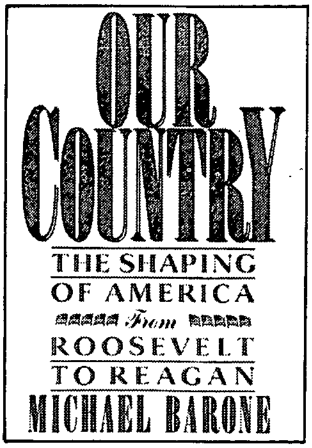

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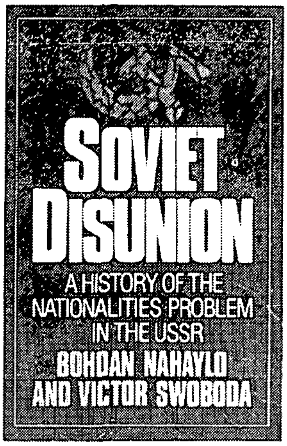
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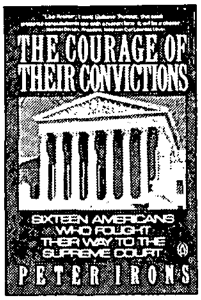
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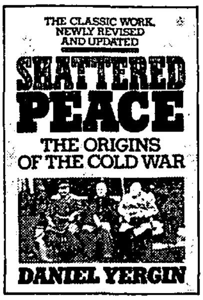
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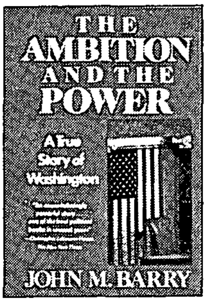
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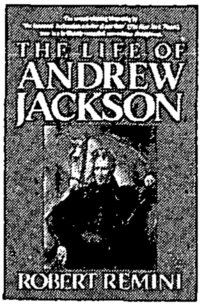

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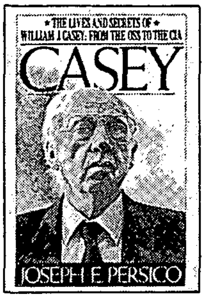

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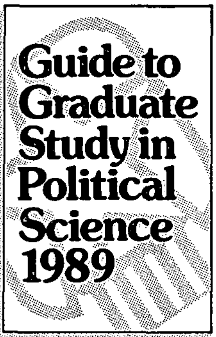
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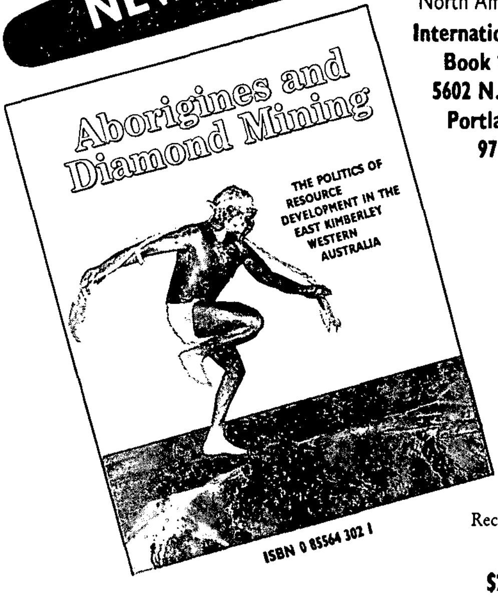
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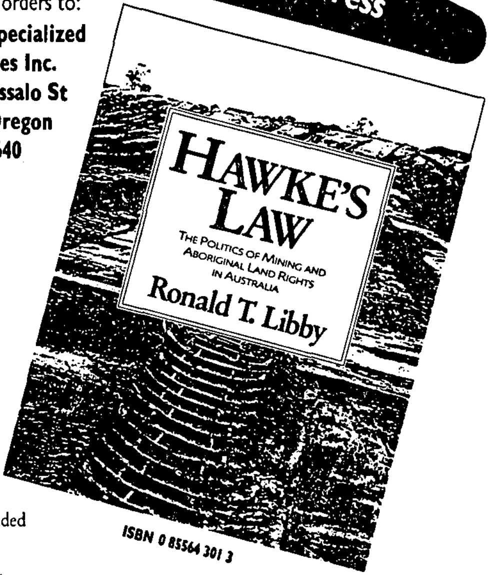
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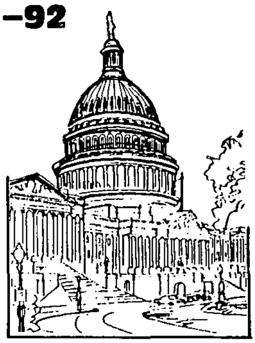

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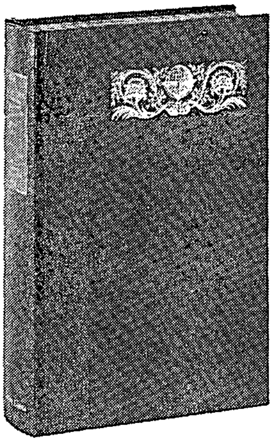

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